923

Philosophy of Science

VOLUME II

BALTIMORE, MARYLAND

1935

Reprinted with the permission of The Williams and Wilkins Company.

JOHNSON REPRINT CORPORATION
111 Fifth Avenue, New York, N.Y. 10003

JOHNSON REPRINT COMPANY LTD.
Berkeley Square House, London, W.1

First reprinting, 1966, Johnson Reprint Corporation
Printed in the United States of America

Philosophy of Science

VOL. 2 January, 1935 NO. 1

CONTENTS

The Nature of Philosophic Analysis. Albert E. Blumberg - - - - - - - - - - - - - - - 1

Dialectics versus Mechanics. A Communist Debate on Scientific Method. A. Emery - - 9

What is Topology? Philip Franklin- - - - - 39

Methodology of Modern Physics. Henry Margenau - - - - - - - - - - - - - - 48

The Biophysics of Space and Time. N. Rashevsky- 73

The System of the Sciences and the Organization of Knowledge. Henry Evelyn Bliss - - - 86

Correspondence- - - - - - - - - - - - - - 104

Reviews and Notes - - - - - - - - - - - - 115

PHILOSOPHY OF SCIENCE

ORGAN OF THE PHILOSOPHY OF SCIENCE ASSOCIATION

The Editorial Board

WILLIAM MARIAS MALISOFF, *Editor*
Philadelphia, Pa.

ERIC TEMPLE BELL
ALBERT E. BLUMBERG
RUDOLF CARNAP
MORRIS RAPHAEL COHEN
W. W. COOK
HERBERT FEIGL
K. S. LASHLEY
HENRY MARGENAU
H. J. MULLER
L. SUSAN STEBBING
DIRK J. STRUIK
ALEXANDER WEINSTEIN

The Advisory Board

EDGAR ALTENBURG
G. D. BIRKHOFF
MARCEL BOLL
P. W. BRIDGMAN
K. T. COMPTON
E. U. CONDON
W. J. CROZIER
L. C. DUNN
ALBERT EINSTEIN
R. A. FISHER
J. B. S. HALDANE
R. G. HARRISON
H. HENCKY
H. S. JENNINGS
K. KOFFKA
W. KÖHLER
K. LANDSTEINER
I. LANGMUIR
RALPH LILLIE
A. O. LOVEJOY
W. MAYER
W. P. MONTAGUE
T. H. MORGAN
J. VON NEUMANN
F. S. C. NORTHROP
H. A. OVERSTREET
M. POLANYI
G. Y. RAINICH
N. RASHEVSKY
H. REICHENBACH
O. L. REISER
O. K. RICE
A. E. RUARK
GEORGE SARTON
M. SCHLICK
C. E. SEASHORE
H. SHAPLEY
E. A. SINGER, JR.
HENRY B. SMITH
W. F. G. SWANN
R. C. TOLMAN
H. UREY
M. S. VALLARTA
PAUL WEISS
GERALD WENDT
HERMANN WEYL
A. N. WHITEHEAD
N. WIENER
E. WIGNER
SEWALL WRIGHT
F. ZWICKY

Copyright 1935, The Williams & Wilkins Company

Philosophy of Science

VOL. 2 January, 1935 NO. 1

The Nature of Philosophic Analysis[1]

BY

ALBERT E. BLUMBERG

THE purpose of this paper is to examine briefly what is meant by the term "philosophy" as it occurs in such expressions as "philosophy of physics," "philosophy of mathematics," "philosophy of science" and the like. The discussion will be divided into three parts: the first will consider several distinct meanings of the term "philosophy;" the second will discuss one of these in some detail, namely, philosophy as analysis; the third will seek to answer an important question concerning the results of philosophic analysis.

I

Philosophy, in the popular sense, appears to be an attempt to express one's attitude towards "life" and its problems. It is in this sense that anyone is a philosopher who so much as voices the questions—What does life mean? Why am I here? and so forth. Reflections on such problems often possess therapeutic efficacy for the person who makes them. Sometimes they are of

[1] Read, in part, at a meeting of the American Philosophical Association, Bryn Mawr, Pa., in December, 1932.

interest to the general public. But as a rule philosophy in this sense consists of aesthetic, personal documents which reveal much more about the author than about life. No one, of course, will deny that the object and aims of such reflections are significant. The point is rather that the questions to which they address themselves, if they are to be answered at all, will be answered by the sciences and technical philosophy.

The expression "technical philosophy" has itself at least two distinct meanings. In the first place, it designates a specific group of subject-matters, the traditional branches of philosophy: metaphysics, epistemology, and theory of value. In the second place, it designates not a subject-matter but an activity with respect to subject-matters. This activity is that of analysis or clarification of meaning, and it is in this sense that one speaks of the philosophy *of* things rather than simply of philosophy. Analysis, as the attempt to clarify the meaning of basic terms and assertions, may occupy itself with any subject-matters, including those of traditional philosophy. Where the latter is the case one may speak of the philosophy of philosophy.

It may be desirable further to illustrate the distinction between activity and subject-matter. Physics is a science and possesses, roughly speaking, a determinate subject-matter. The results of physical investigations are physical laws which describe or explain the facts which constitute the subject-matter of physics. Philosophic analysis, on the other hand, is not a science. It possesses no distinctive subject-matter. The results of philosophic investigations, therefore, are not "philosophic laws" but rather the clarification of the meaning of the analyzed statements. Consequently, the term "philosophy" as it occurs in the expression "philosophy of science" denotes not a subject-matter but an activity, that of clarifying the meaning of basic terms and assertions in the sciences.

It is clear that all philosophers have practiced analysis in one form or another, at one time or another. But it is only recently that some of them—G. E. Moore, L. Wittgenstein, M. Schlick, R. Carnap and their followers of the Cambridge and Vienna schools—have gone so far as to maintain that analysis is the dis-

tinctive function of philosophy. The Vienna group, following Wittgenstein, Schlick and Carnap, go a step further and assert that analysis is the *sole* function of philosophy. They contend that the assertions which make up the traditional subject-matters of philosophy are either (1) statements of a scientific character, whose truth or falsity is therefore to be investigated by scientists, or (2) meaningless statements, whose meaninglessness can be demonstrated by philosophic analysis. It is not the purpose of this paper to defend this point of view, nor to examine more closely into possible jurisdictional disputes as between philosophic analysts and scientists. We pass then to a more detailed account of the nature of philosophic analysis.

II

Philosophic analysis is the attempt to clarify the meaning of assertions. The purpose of this section is to help clarify philosophic analysis by means of a discussion of the objects, objective, method and results of analysis.

Since the objects of analysis are assertions, the first step in the clarification of analysis is an examination of the chief types of assertions. There are two basic groups: assertions of syntax and assertions of fact. This dichotomy follows from the nature of language. The world consists of facts. Some facts and elements of facts serve as signs, that is, they stand to someone for other facts and elements of facts. A language is a set of signs, but in addition a language requires a syntax or body of rules which prescribe the way in which the signs may be combined into statements to express facts. Thus, two types of statements can be formulated: syntactical and factual. The former prescribe the use of symbols; the latter express, refer to or indicate facts. The correct determination of the syntactical or factual character of a given assertion is an essential prerequisite to its analysis. This determination can not be effected by viewing the assertion in isolation. One must examine the definitions of the terms which compose the assertion. If the terms have been defined independently of one another, the assertion is factual; if not, it is syntactical. Analysis reveals that definitions, tau-

tologies, equations (in pure mathematics), conventions (e.g. in physics), are statements of syntax. Propositions, hypotheses and scientific laws are statements of fact.

The objective of analysis is the clarification of the *meaning* of assertions. Consequently, the second step in the examination of philosophic analysis is the clarification of "meaning." This involves to begin with a distinction between meaning and significance. The meaning of a sign is the referend which it signifies; the significance of a sign is its emotional and imaginal associations and effects. In this strict sense of the term "meaning" only signs have meaning, and the meaning of a sign is simply that which it stands for or signifies. Secondly, one must distinguish between the "meaning" of a statement of syntax and the meaning of a statement of fact. Statements of syntax convey information about signs, not about the referends of signs. Hence, statements of syntax are more accurately designated as *pre-meaningful.* Statements of fact, on the other hand, convey information about facts. Their meaning or referend, is, then, the fact or set of facts which they assert. Philosophic analysis may be said to be concerned primarily with statements of fact since (a) most statements are statements of fact, and (b) the analysis of statements of syntax is, on the whole, relatively simple.

Analysis, the clarification of meaning, is a methodical, directed process, not a random listing of items suggested to someone by a given statement. The method and direction of the analysis of factual statements emerge most clearly if meaning is stated in terms of the process of verification. (If one wishes to stress the quite plausible position that few—perhaps no—statements of fact are capable of final, complete verification, one may substitute throughout this discussion the term "confirmation" for the term "verification.") For a factual statement to have meaning it must be at least theoretically capable of verification. Its meaning is the set of facts given in the process of verification. Hence, to clarify meaning is to trace this process. Thus analysis seeks to set forth those facts which will be the case if the assertion under analysis is true. The direction of analysis is, therefore, the direction of the process of verification. Verification proceeds from

complex to simple in that it approaches through a series of definitions the simple, ultimate facts upon which the truth of the given assertion is based. The process terminates with the recording of these simple facts. Correspondingly, an assertion is completely analyzed when it has been completely translated into the basic or atomic propositions which express the simple, ultimate facts determined by the verification of that assertion. (It must be added, that if one holds the arguable position that there are no absolutely atomic propositions, the foregoing statements may be suitably amended by taking the terms "simple," "ultimate," "basic," and "atomic" in a relative and not an absolute sense, and by adding the qualification "at a given level of atomicity" to the expression "completely analyzed." The sentence immediately preceding this parenthesis might then read: An assertion is completely analyzed at-a-given-level-of-atomicity when it has been translated into the atomic-propositions-of-that-given-level-of-atomicity which express the facts-of-the-corresponding-degree-of-simplicity determined by the confirmation or verification, to the given level of the assertion in question.)

The objective of analysis is the clarification of meaning; accordingly, the result of a successful analysis would appear to be a definition, that is, a statement which fixes the meaning of the term or assertion under analysis. More exactly, it is a statement to the effect that the assertion in question is completely translatable into or means the same referends as the specific set of basic propositions which constitute the terminal point of the analysis. Definitions are statements of syntax, conventions concerning the use of signs. To avoid misunderstanding, definitions must be sharply distinguished from so-called "real definitions." The latter, as in the case of Aristotle's "Man is a rational animal" are composed of terms defined independently of one another and are therefore statements of fact. The "real definitions" assert generalized mutual implications or equivalences. Consequently they relate to facts, not to the use of signs. Such statements are then true or false depending upon whether the facts asserted are or are not the case. Definitions, on the contrary, assert nothing with regard to facts, are therefore incapable of being true or

false, and must be regarded simply as conventions or agreements concerning the use of signs.

III

The preceding rapid survey of the objects, objective, method and apparent results of analysis serves as an introduction to the problem: What is meant by a *correct* analysis? The problem arises in the following manner. The results of analysis appear to be definitions, but definitions are conventions not capable of being true or false; hence the expression "This is a correct (or incorrect) analysis" seems to be nonsense. Yet there is certainly a sense in which one can discuss the merits of analyses, reject some as "incorrect" and accept others as "correct."

In short, it seems that the conventional character of definitions is incompatible with the notion of correct (or incorrect) analysis. This apparent incompatibility has induced some authors to reject the view that definitions are conventions. Thus, it has been suggested that a definition may be termed true if it corresponds to "correct usage." A correct analysis could then be defined as one which yields a "true definition." But even apart from the difficulties attached to the expression "correct usage," this view of definitions overlooks the circumstance that what is true or false in this case is not the definition, but the factual assertion: "This definition corresponds to correct usage." The definition as such remains a statement of syntax, a convention concerning the use of signs.

This mistaken view of definitions does, however, give us a clue to an answer to the problem—an answer in which the conventional character of definitions is shown to be compatible with the notion of correct (or incorrect) analysis. To analyze the meaning of an assertion is to trace the process of verification. Verification is a human activity. People employ signs and verify assertions. Hence, to analyze is to determine *how* (in the sense of *for what referends*) a person or group of persons actually use a given sign or set of signs. If this is clear, then it follows that the result of an analysis is not a definition but a factual statement in which reference is made to a definition. The factual statement is of

the type: *This person or group of persons employs this sign or set of signs in the way stated by this definition.* The factual statement, in short, asserts that the definition corresponds to or accurately renders the usage examined. And the analysis which yields that definition will be *correct* if the definition does accurately render the usage examined. Analysis thus yields statements about the linguistic habits of people. Analyses will be correct if the people in question actually use the signs in the manner asserted. It follows, of course, that where various people employ the same sounds or marks for different sets of referends, the several analyses of the different usages may all be correct at this initial *descriptive* level of analysis.

But philosophic analysis is generally assigned the task not only of describing the uses of signs, but also of prescribing usage. At this, the *prescriptive* level of analysis, the expression "correct analysis" takes on a different meaning. By it is meant a sustained process of analysis which yields a single, superior usage. Prescriptive analysis attempts to answer the question: *In what way ought people use a given sign*? as distinguished from the question raised at the descriptive level: *In what ways do people use a given sign*?

The passage from description to prescription is difficult in practice, simple in theory. There is no such thing as *the real meaning* of a sign. Signs mean whatever people make them mean. Hence given a complete, correct, descriptive analysis of the uses of a particular sign, the selection of one among them as superior (as *the* meaning of the sign) can be based only upon considerations of utility, convenience and aesthetic value. One must be able to show, for example, that to use sign A for referends *m* and *n* is inconvenient, since there is a perfectly good sign B which is already so used. Similarly, for the other uses of sign A the advantages and disadvantages are to be weighed, taking into account naturally the relationship of these uses to relevant uses of other terms. Finally, by a process of elimination, a single usage is determined and analysis prescribes it. Not until this point is reached can one speak of *the* meaning of a sign, *the* correct analysis of an assertion.

This account of philosophic analysis retains the conventional character of definitions (which rests ultimately on the fundamental distinction between statements of syntax and statements of fact), and distinguishes and provides for two meanings for the expression "correct analysis." In this process, analysis is transformed from a vague, exalted, mystical "pursuit of meaning" into the concrete, practical activity of describing the ways in which people use signs, with a view where feasible to prescribing single, superior usages.

Department of Philosophy,
Johns Hopkins University,
Baltimore, Md.

Dialectics versus Mechanics

A Communist Debate on Scientific Method

BY

A. EMERY

"A millennium has gone by since the idea of the relationship of all things, the chain of causes was born. A comparison of the meaning of what we call causes throughout the history of human thinking could give us, no doubt, a conclusive epistemology." (Lenin, V. I., *Unpublished Notes to Lasalle's Heraclite.*)

IN DECEMBER 1929 Stalin addressed the Conference of Marxian agrarian scientists and made the general accusation

"that there is a certain hiatus between our practical achievements and the development of theoretic work which should not merely keep up with the practical but indeed precede it by arming our practitioners in their struggle for the victory of Socialism."

Immediately the Philosophic Section of the Institute of Red Professors issued a call for a radical change on the "philosophical front." The focus of the change was to be the attitude toward the problem of theory and practice. The old philosophic leadership (A. Deborin especially) was accused of formalism which ignores vital questions of the day. It was expressed by M. Mitin of the the new front with the following words:

"In formalism dialectics is transformed from a vital method of cognition into a coördination of abstract formulae, which are applied to or fitted to the content entirely from the outside. Formalism consists in

the break which occurs in theoretical work between the form and its content, where the logical is separated from the historical, where the philosophic theory is transformed into a sequence of ideas altogether separated from the complete historical, social, class situation. (Hecker, J. F., *Moscow Dialogues*, pp. 184–185.)

The recognition that the categories of knowledge are not fixed and unchangeable but are subject to the process of dialectical movement (a notion emphasized by Engels and Lenin), and consequently a conscious desire for a living theory which leads the way instead of hobbling behind a rule of thumb practice were the outcome of a long and bitter debate which lasted through the groping and tempestuous times of 1924–29. The slogan "philosophy overboard—science is all sufficient" was the expression of a generation deeply engaged in empirical experimentations both in economic and scientific life. One hardly could accuse these revolutionaries of "dogmatic Marxism"; they were busily engaged in disregarding the philosophical heritage of their classical masters.

I. Stepanov was the leader of this group of "mechanists" which threatened at times to capture the ideological leadership of the communist party. The popularity of this tendency was due partly to its simplicity and emphasis on the empirical sciences, partly however to the general optimism of the years 1924–1929 when the temporary stability of capitalism in the Western world and the New Economic Policy (NEP) in the Soviet Union lulled the people to believing in a painless and uninterrupted evolution of whatsoever they were prepared to see developing.

However, very little material on this controversy was published in English. Most of it therefore is available only to those who are acquainted with the Russian language, and so we are limited as to our sources.

In April 1929, an all union conference of representatives of the scientific Marxist-Leninist institutions was called to Moscow. In this conference the finals of the debate between the mechanists and the opposing dialectical school was held. Due to the traditional revolutionary meaning of the word "dialectics," it was natural that the mechanists called themselves disciples of Marx, Engels and Lenin and dialectical materialists, but the opposition

claimed that their dialectical terminology was either meaningless or unwarranted. The leaders of the mechanists—since Stepanov died shortly before the conference—were A. K. Timiryasev and V. Sarobyanov. They insisted upon the independence of the natural sciences apart from any philosophy and proposed to replace philosophy by general conclusions of the natural sciences. The opposition was led by A. Deborin—who a year later was condemned by the Adoratsky-Milyukov-Mitin group on the ground of formalism, but whose merits in the mechanics-dialectics debate were recognized.

"The mechanists"—said Deborin—"stand on the basis of bare facts, revealing an incomprehensible fear before theoretical thought, but we dialectitians 'tie' facts with theory, understanding them as a unity and as a relation." (Hecker, J. F., *Moscow Dialogues*, p. 172.)

In the centre of argument stood the problem of the reduction of phenomena to the common denominator of the simplest term. The mechanists do not recognize the hierarchy of sciences and believe in the possibility of a reduction of the complex laws governing the behavior of society and living matter to the simpler physical-chemical laws and these in turn to the most simple laws of mechanical motion.

The mechanists are in straight lineage with the French materialists of the 18th century, who in turn were influenced by Newtonian physics. Most of the popular French and English positivists up to today are close kin to them, in spite of the coating of agnosticism. The position of the mechanist reaches back (as Stepanov himself admitted) to the old Pythagorean idea of fixed quantity or number being the essence of things. The dialectitians charge therefore that the mechanist theory is, in spite of all its claims to dynamics, an essentially static and metaphysical theory. Because of this static and metaphysical quality it often borders in its conclusions—as paradoxical as it might sound—on the indeterminist idealist philosophy.

The dialectitians on the other hand consider the world process as a creative and synthetic one. The dialectic method was used by natural scientists from Leibniz to Einstein for creative think-

ing, but rarely as a conscious method and was usually based upon idealist premises. F. Engels in his posthumeously published book: *Dialectics of Nature*, showed how materialist dialectics can be used as a method to synthesize the fragmentary conclusions of the natural sciences, thus paving the way for new departures.

Dialectic materialism is a synthesis of two lines of philosophy. It is the union of a materialism which was not dialectic (Hobbes, Spnoza, the French Enlightenment, etc.) with a dialectics which was idealistic (Aristotle to Hegel). Engels attacks the materialists of the 18th and 19th centuries for attempting to reduce complex organic and social phenomena to physics and chemistry. He admits that there is a transition, a contact between the biological and the physio-chemical, the higher forms having lower as antecedents, but maintains that the higher form is inevitably a new thing, which cannot be explained by the simple sum of the "elements." Lenin followed Engels closely and warned against the metaphysical pitfalls of postulating as a "common denominator" any unchangeable, fixed "brick of the universe."

There are many eclectic tendencies ranging from indeterminism to actual mechanism both in the Soviet union and in the western capitalist world, and therefore the debate was not terminated with its official conclusion at the conference of 1929, where the dialectic school had a full victory. E. Kolman, Professor of the Institute of Mathematics and Mechanics in Moscow, forecast the future in the following words:

"The protracted and painful labor through which the natural sciences today are giving birth to dialectical materialism strengthens our conviction that without the midwife of history, the revolution, the matter will not proceed, that it is only the new generation of proletarian investigators of nature, liberated from the ideological slavery of capitalism, that will finally destroy the old traditions of metaphysical methodology that now hamper science; for the essential reason that prevents the bourgeois investigator of nature, the spontaneous materialist, from recognising dialectics and which, even in the period of capitalist decline urges him towards idealism, is his close association with the ruling class and their ideology." (Kolman, E.: Dynamic and statistical regularity in physics and biology. *Science at the Crossroads*, p. 90.)

To gain insight into the subtle and far reaching philosophical and scientific meanings of the controversy, we propose to follow the arguments of Ladislaus Rudas, professor of philosophy at the Lenin University[1] against N. I. Bukharin, member of the Academy of Sciences and editor of the *Izvestia*, who in spite of his high position was branded during the controversy as a mechanist. We shall use Professor Rudas's arguments freely without quoting him at every instance.

I. THE CONCEPT OF FORCE

A. The mechanical viewpoint

> "If, however, movement is the primary change underlying all other changes in the world, all elemental forces are forces of movement and the aim of the natural sciences is to find the movements underlying all other changes and to find their motive power, that is, to resolve them into mechanics." (Helmholtz, H., Address at Innsbruck, 1869.)

Force in mechanics is the means of explaining the causal relationship of the world. The mechanist is convinced that he has penetrated and explained the phenomena of mechanical movement when he has indicated some "force" as a cause. The mechanist image of the world is described by Rudas about as follows:

There is absolute space and absolute time. In this space there are an infinite number of material points separated from each other by the absolute space. The space is merely a stage on which the drama is played, and in no other way does it participate in the world process. It is motionless, inactive. Each point in the space exerts an influence upon all other points. Movement is caused by force and it is proportional to the effected acceleration. The material points are linked together by such forces acting directly through the distance (Fernwirkung). But however much the dividing space might be reduced, it can never be entirely eliminated. The forces might be arranged in pairs, and if these are equally large in magnitude but opposite in direc-

[1] Rudas, L.: Ueber einige Grundbegriffe der Mechanik und der Dialektik, and Rudas, L.: Dialektische Theorie der Kausalitaet. *Unter dem Banner des Marxismus*, Jahrg. IV, Heft 3, resp. Jahrgang III, Heft 6.

tion, (the principle of equality of action and reaction) they hold each other in equilibrium. This opposition of the acting and reacting forces, this antagonism, is the only active contradiction in the world. Material bodies are systems of material points held in equilibrium by antagonistic forces.[2]

[2] "As long as physics followed the road which was pointed out by Newton, the following image of physical reality prevailed: matter is real and is subject only to changes which can be conceived as changes in place, in space. Movement, space, and time are real forms. Each attempt to deny the reality of space is refuted by the phenomenon of inertia.

In Newton's system there was a third fundamental reality: forces which act between the material particles and which depend exclusively on the relative situation of these particles. These forces were integral properties of the particles and distributed in space according to an immutable law.

The physicists of the 19th century assumed that there are two kinds of matter, concrete matter and electricity. The particles of concrete matter act, according to the Newtonian law, upon each other by the force of gravity; the units of electricity, by the force of Coulomb, both inversely proportionate to the square of the distance. Besides that there was no definite opinion as to the nature of the forces which act between the particles of this or that matter.

The absolute space had no quality whatsoever. It was viewed as 'the stage' upon which the drama of the material particles plays. Accordingly Newton explained the movement of light in empty space with the hypothesis that light also consists of material particles which interact with the particles of heavy matter.

In such a way Newton assumed a third kind of material unit, which however greatly differed from the former two." (Einstein, quoted by Rudas, in *Unter dem Banner des Marxismus* Jahrg. IV, Heft 3, pp. 308–309. My translation. All following quotations, which originally appeared in German are translated by me. A. E.)

"In nature we deal with material bodies having finite dimensions, and not with material points. But we might look upon any body as composed of many material points, and we can reduce the difference in the mechanical properties of the respective bodies to the inequalities of forces with which these individual points act upon each other. Then the problem of the laws of motion of material bodies is reduced to the mechanics of the material points, as systems.

If so, there are no other material forces in nature except those between the material points. Each of the material points moves in conformity with the resultant of the forces which act upon it from all the other points of the universe. If we speak of a force which an entire body undergoes, we do not mean it literally; it is to be understood as an abbreviation. In reality only the individual points of the body are sources and points of attack of the forces. Because each force acts from a given (definite) material point A on a definite material point B.

Therefore all forces of nature might be arranged into pairs, if to each individual force we find the correspondent force, which is exercised by the second point B on the first point A, and if these two corresponding forces are—in keeping with the principle of the equality of action and reaction—equal in size but opposite in direction." (Planck: *Einführung in die Allgemeine Mechanic*, p. 105.)

The material parts of which the world consists are not processes, but atoms in the old sense of the word, rigid, isolated points which have no other attribute besides the forces they possess.[3] Each point can exercise only one force, and therefore, only forces of different points are different in direction. If a hypothetical point would exercise several forces, the mechanist would divide it into several points. This was what happened to the atom. When it was discovered that it is a system of complex movements, the mechanically minded natural scientist went on a search for the "last unit" of the system (electrons, subelectrons, etc.), and the force which holds the system in equilibrium.

If all material points exercise only one force, contradictions can only be external. Therefore no inner contradiction, no unity of opposites, is known in mechanics. All contradictions which seem to be immanent are cut in two, externalized into the conflict of forces embodied in different material particles. This is the heritage of Newton's mechanics.

But the force which acts in one point is external in relation to that point, too. Mechanics recognizes force as the only attribute of the material point, but not as a necessary attribute. It might be abstracted, and matter as such, matter in complete rest, might be conceived. Since rest is taken as the immanent mode of existence of matter, movement has to come from an outside source—force. This is the theory of mechanics from Newton to Planck.[4]

In mechanics absolute space and material points are the last

[3] "The material point has to be distinguished from the geometrical point; the latter is perfectly characterized by its place, whereas the first shows also peculiarities in the nature of its matter (Beschaffenheit). Indeed the material points have to be considered dissimilar both as to quantity and as to quality." (Planck, *Ibid.*, page 2.) This warning was disregarded by the orthodox mechanists.

[4] Newton evaded the question why gravitation is a universal law. He maintained that it is a universal, although not an essential attribute of matter. (See *Philosophia Naturalis*, Vol. III, p. 4.) Because movement—that is its cause, force—is accidental and unexplicable from the viewpoint of mechanics, Newton refused to accept the theory of the conservation of energy. Bodies are systems of rigid atoms and by the clash of such atoms mechanical energy is lost. The sum total of force is in constant diminution, and therefore the world needs from time to time a divine impulse for its preservation. (Newton, *Optica*, p. 332.)

realities. The permanent, formless, passive space, and the material points in it, are hopelessly separated from each other. Likewise the material bodies are held apart by the vacuum, and they can never unite in a real synthesis. Sun and earth, electron and proton of the material world are equally estranged and have to act across the distance directly upon each other. This complete isolation has to be bridged somehow, by something which gives a unity to the world.

Therefore mechanics searches for the force which gave the first impetus to "matter as such," passive matter. It introduces the forces which disturb the equilibrium of the resting bodies. According to Einstein, the concept of force in mechanics is characterised as the independent prime factor which underlies all physical reality. It acts between the material particles of the world and depends exclusively upon the distribution of these particles in space. Mechanics has not yet clarified however what the nature of this force is.

B. Bukharin's concept of force

> "Antagonism and contradiction are not the same by far. The first disappears, the second remains in socialism." (Lenin, V. I., *Collected Works*, vol. XI, p. 357.)

"The internal (structural) equilibrium is a quantity which depends on the external equilibrium, is a 'function' of this external equilibrium." (Bukharin, N. I., *Historical Materialism*, p. 79.)

"The 'conflict,' the 'contradiction,' i.e. the antagonism of forces acting in various directions, determines the motion of the system." (*Ibid.*, p. 74.)

"Hegel said: 'Contradiction is the power that moves things.' There is no doubt of the correctness of this law. A moment's thought will convince the reader. For, if there were no conflicts, no clash of forces, the world would be in a condition of unchanging, stable equilibrium, i.e. complete and absolute permanence, a state of rest precluding all motion. Such a state of rest would be conceivable only in a system whose component parts and forces would be so related as not to permit of the introduction of any conflicts, as to preclude all mutual interaction,

all disturbances. As we already know that all things change, all things are in 'flux,' it is certain that such an absolute state of rest can not possibly exist. We must therefore reject a condition in which there is no equilibrium only absolute immutability." (*Ibid.*, p. 73.)

Bukharin's "dialectical" sociology is astonishingly mechanical in its pattern. Contradiction and antagonism are used identically. Contradiction is reduced to a conflict of forces operating in two alien systems. In his social theory these two alien systems are nature and society. The interaction of life and its environment are the two forces which strive to establish an equilibrium but never interpenetrate. Bukharin's image of the world-process begins with a hypothetical equilibrium. Any change of the forces which are in "internal contradiction," (a meaningless expression since he knows only mechanistic collision) disturbs this equilibrium. The readjusted equilibrium is a new basis for the further process.

In his political theory Bukharin pictured the development of socialism in Soviet Russia as a "moving equilibrium," started on its path by the revolution. He substituted for the inner contradiction of the capitalist system—which was Marx's theory—an equilibrium of organized capitalism, which can be upset only by external conflict, by the international rivalry for markets. These and some other opinions brought him into temporary disfavor in the communist party. But when the years of theoretical uncertainty (1924–1929) were past, Bukharin recanted his views and his position was reëstablished in the party.

The dialectitians attack Bukharin on the ground that his theory of equilibrium differs fundamentally from the theory of immanent motion, the unity of opposites. He recognises only quantitative changes in forces, neglecting the qualitative changes which make the appearance of new and higher functions possible. (With the last argument we shall deal in the second part of this article.)

C. The dialectic universe

> "Because we do not see quite clearly the 'rather complex conditions' of a phenomenon, we take refuge in the use of the word 'force.' By this however, we do not express knowledge, but a lack of knowledge concerning the nature of the phenomenon

> and the laws of its functioning. In this sense, as an abbreviation for a not yet clarified causal relationship, as an emergency measure of the language, it can pass in common usage. If however, it wants to mean more it is harmful." (Engels, F., Grundformen der Bewegung. Marx-Engels *Archiv*, II, p. 246.)

Matter and motion are important categories on which mechanists and dialectitians differ. The latter hold that matter is inseparable from motion. No matter exists without motion. Motion is a basic attribute, a conditio sine qua non of the existence of matter. The mechanists on the other hand understand motion only as a change of place, as a mechanical process started by application of external forces. Mechanists and dialectitians differ also in their understanding of the nature of matter. To the mechanist matter consists of some final indivisible element, electron, proton, or any other brick of the universe, whereas for the dialectitian as Lenin said "matter is objective reality existing independent from human consciousness and reflected in it."

The living inner relationship of matter and motion, content and form, relative movement and relative rest, is dissected by mechanics into independent, opposing, individual moments: action-reaction, attraction-repulsion. On one side matter is the embodiment of absolute rest; on the other side force is the absolute cause, —motion being the absolute effect. The world is disintegrated into points distant from each other, and their action over this distance does not become more comprehensible if we reduce the millions of miles which separate the sun from the earth to the infinitesimal space between two atoms. Near and far are relative concepts, and the impenetrability of the absolute space does not become a lesser obstacle to unity if it is reduced in magnitude.

However, modern natural science realizes this clearly.[5] The mechanistic world picture is outdated and discarded. The atom resists the scientist who wants to solve the problem of its motion mechanically. The electron refuses to be considered exclusively

[5] "In spite of the fact that it took the painful work of many centuries to make the image of a direct action through distance a living habit (of thought, A. E.) we have to succeed in stripping it off again if . . . this image has served its purpose." (Planck "*Das Prinzip der Erhaltung der Energie*, p. 274.)

as a material point and to follow the oldfashioned laws of mechanics. Its behavior can be explained only by theories which admit an inner unity of opposing forces and which defined the electron as a junction in the aether, uniting the properties of concrete matter with that of the wave. Mechanics is surpassed.[6]

Modern natural science is far from a complete world outlook. But the characteristic traits are already recognizable, and they are sharply opposed to mechanistic philosophy. The image of the world which it presents differs considerably from the one we described as the mechanistic universe. Rudas depicts it as follows:

> There is no absolute space, nor absolute time. Time-space is closely connected with and dependent on matter and its motion. Space and time are functions of the motion of matter, therefore they are always relative. Each specific concrete movement is a relative one, although motion is a prime attribute of matter. Space is not a passive stage for the world-drama, bare of qualities, but a mode of existence of matter, which changes in accordance with the motion of matter. Time changes similarly. Physical phenomena can not be reduced to simple changes in the spacial distribution of material points and bodies. These material bodies and particles (electrons, protons, atoms, molecules, bodies, stars, etc.) are indivisibly united with the aether, from which they originate by condensation, by compression to points of juncture (Knotenpunkte), and with which they are in continuous interaction. In consequence there are no "forces" to bind the isolated particles together and to cause their motion. Aether and quanta, concrete matter (whether atom or star) constitute a continuum which is not built from isolated individual points as mechanics assumes, but which differentiates itself, without losing its unity, into special kinds of matter and movement.

Thus, the category of external force is eliminated and the explanation of causation becomes less simple. Matter, movement, space and time are one continuum, a dialectical unity of content and form. Space and time are forms of existence of matter. Movement is an immanent quality of matter.

[6] ". . . it is absolutely superfluous and incorrect to assume that each physical oscillation which originates from the periodical change of some magnitude, is a mechanical oscillation." (Haas, A., *Materiewellen und Quantenmechanik*, p. 178.)

From the whole of the world process special events originate without losing their unity with the whole. Each individual phenomenon, everything we perceive, is such a special process. The perceptible matter which mechanics calls material point is merely a point of juncture of the non-immediately perceptible continuum. Discrete matter (electrons, protons, atoms) is originated by processes reflecting the inner dialectical contradiction of the unified permanently moving universe.

Since force is not the cause of motion, motion appears to the perceiving mind which isolates phenomena, sometimes as cause and sometimes as effect. But dialectics can reconcile the relative conditioned character of the individual movement and the absolute character of motion as such (absolute: meaning without cause). The mechanist thinker can not explain the "force" foreign to the matter in which it houses. He does not know from where force comes; it has always to be cause and never effect. Because he assumes that rest and not motion is the original state of matter, every mechanist like Newton has to presume a first impulse. This leads to God-building. Even in Bukharin's writings we find such a first force: nature, which gives a continuous impulse to the development of society.

The category of force is neither accepted by dialectic materialism nor by modern science. It is legitimate only as a figurative expression, an emergency measure of the language, to express not yet clarified causal relationships.

Long before natural science accepted these views of the inner unity of opposites, dialectics had professed them.

"The essence of movement is the immediate unity of space and time, time and space belong to movement. . . . Space and time are filled with matter. . . just as there is no movement without matter, there is no matter without movement." (Hegel, *Natur-Philosophie*.)

Maxwell was one of the first physicists to advance the hypothesis of a unified universe. The field theory which he originated is called by Rudas the illegitimate child of Newtonian physics. The field which takes the place of the "absolute space" is not merely a stage for the world-drama, it is indivisibly connected

with the concrete matter in the dynamic process. By mechanistic theory force acts from separate point to separate point across the inert vacuum. The field theory disputes this permanent isolation. Between concrete particles there is always a field, and the bodies do not act immediately upon each other but forward the movement to the surrounding field, and this carries the effect on to the next concrete body. With this hypothesis the basic concepts of Newtonian mechanics are challenged.[7]

Since there is no empty space, and the field is an active, dynamic process in constant and unified interaction with the "material points," the seemingly irreconcilable opposites of "empty" and and "full" find a synthesis in a general and unified concept of the universe. The "full" originates from the empty dialectically; the empty is only relatively empty, that is, inasmuch as it has different qualities from the "full." Electrons, protons, concrete matter in general are only a special form of the empty (the aether) and not "elements shifted to and fro by aggressive forces."

This unity of aether and material particles, that is the indivisibility of the universe is a new point of departure in modern natural science.[8] Quantum mechanics extends the understanding of this unified world.[9] The specific discoveries of modern natural

[7] "One can not call this any more a dynamic world image, because the field here is neither produced by a factor (Agens) opposed to the field, nor does it act upon such a factor; it follows solely its own laws and remains in the state of a quiet continuous flow. It is at rest in continuity: even the nuclei of the atoms and the electrons are not—as last and unchangeable elements—pushed by the aggressive forces of nature to and fro, but are in continuous expansion and subject to fine, flowing changes." . . . "There can be no empty space; the assumption that the field leaves a place in space free is absurd." (Weyl, H., *Philosophie der Naturwissenschaft*, p. 130–131.)

[8] "In the beginning of this century the physicists began to see the insufficiency of the dualistic theory which presumed two kinds of basic realities: on the one side the field, on the other the material particle. It is natural that there were attempts to imagine the material particles as belonging to the structure of the field, i.e. as places in which the field is unusually condensed." (Einstein, *Ibid.*)

"As our concept of space and time becomes clearer the more we see that everything to which our theory of dynamics refers is connected in one unique system." (Maxwell: *Matter and Motion*, p. 92.)

[9] "The problem of interaction of the electrons and aether assumed another form than it had at the time of the theory of Lorentz, when the electrons so to say swam as strange bodies in the aether." (Jordan: "Character der Quantenphysik. *Die Naturwissenschaften*, H. 41.)

"The specialty of quantum mechanics consists of the fact that it does not make a

science are in accord with the dialectical world picture.[10] Although Einstein, and mystics like Eddington, express the theory of relativity in a geometrical form, determining the movement by the form of the space-time continuum, instead of using the materialist notion that immanent motion of matter determines the space-time, it does not violate the essential value of the theory. Nature will force the physicists to reconsider their theories according to materialist logic.

It is clear from this argument that the mechanist and materialist point of views can hardly be reconciled. We shall analyze this difference as it appears in the problem of the whole and its parts. For this we have to understand the concept of system.

II. THE CONCEPT OF THE SYSTEM

> "As we review history and seek further and further back, the individual—even the producing individual—appears less and less independent. He belongs to a larger unit. First in a natural way to the family, then to the enlarged family, the tribe; later to the different commonwealths which ensued from the conflicts and combinations of the tribes. Only in the 18th century, in 'bourgeois society' did the different forms of social

---

definite choice between two concepts which both can claim recognition (particles and waves), but that it reëstablishes the one after the apparent victory of the other and unites both into a higher synthesis." (Born: Über den Sinn der physikalischen Theorien, *Ibid.*, H. 6.)

"Typical for the most modern physics is that it eliminates the contradiction between concrete matter and waves, consequently between matter and light. . . ." (Haas: *Materiewellen und Quantenmechanik*, p. 123.)

"The contradiction between matter and light (in the broadest sense of the word) was mitigated by the newest development of the atom theory. The most essential distinguishing characteristics of matter, before all the discontinuity and the presence of a quantity of motion are proper also to light, and vice versa, the properties of wave, characteristic of light are also true of matter. In modern physics matter and wave are not the basic concepts of different compartments of the science, on the contrary they present complementary traits of the absolute general and unified cognition of nature." (Naturerkenntnis, *Ibid.*, 178.)

[10] ". . . Heisenberg and quantum mechanics as a whole, correctly emphasize the existence of mutual interaction in any actual process, but cannot by themselves tackle the problem it raises . . . it is clear . . . that the methodological root of the error lies in the undialectical conception of the relation between the general and the particular." (Kolman, *Science on the Crossroads*, p. 87.)

> relationships appear to the individual as simple means for advancing his private purposes, as external necessity. . . . Man is in strict sense a zoon politikon, not only a sociable animal, but an animal which can individualize itself only in society." (Karl. Marx, *To the Critique*, etc., p. XIV.)

According to the mechanistic world-image, the original state of the universe is disintegration, not unity. Material points are connected temporarily by an equilibrium of forces but a real synthesis can never be achieved. The individual points are segregated by absolute space. Consequently there can be no unity in the world at all, only a side by side of parts and particles. New combinations might appear if the equilibrium is disturbed, and individual parts will secede, but nothing new originates. There can be only a regrouping of the old, which has been here forever. Evolution is repudiated.

This univocacy of natural phenomena as a premise annuls the qualitative difference between the general and the particular, and the relationship becomes a purely quantitative one. The whole is equal to the sum of the parts and nothing more. The law of the whole is reducible to the law of the parts which constitute it. The problem, however, is not whether things have integral parts or not; the real question is what the origin and character of these parts is. Dialectics maintains that the parts are not prior to the whole; on the contrary, the character of the whole determines the character of the parts. There is a fundamental difference between antecedent factors, and integral parts.[11]

Man is composed of cells, cells of chemical molecules, the molecules of atoms, these of electrons and protons, etc. Wherever we look we get the same sequence. But—and this is disregarded by the mechanical point of view—things composed from the same elements may be things of different quality. This difference in

[11] Rudas uses as illustrations the theories of Nernst and Fermi on the energy and pressure at the absolute zero and of the Compton effect. This latter he considers as extremely important not only because it proves that motion is an immanent quality of matter and as such is indestructible, but also because it satisfies the law of the conservation of energy and movement in the event of the clash of one electron and one light-quantum and as such establishes proof for the law of causality. He uses Haas: *Materiewellen, etc.*, p. 179 as reference.

quality, which leads to the originality of the aggregate entity is from the dialectical point of view the essential fact and not the existence of a "common denominator." An atom is composed of electrons and protons; but the atom is itself again a unity. As such it is qualitatively different from the protons and electrons which compose it. It is a chemical element, hydrogen, helium or something. As a chemical element it acts according to specific laws. It is a composite part of the molecule and this again is a new entity, qualitatively different from its atoms. In this consideration it might be a molecule of water or any other chemical matter. The cells are composed of chemical matter, that is molecules; but as cells they are a unity of a higher order, obeying different laws. Each of these new entities is a dialectical junction, qualitatively different from their antecedent composite elements, a junction where the old law turns into a new one. The new quality and the new law is neither the sum total nor the continuation, the combination or modification of the old quality and law of the constituent elements. The motion of the electron does not explain the specific laws of the atom, even though they contain as subordinate moments the law of motion of the electron. The molecule again has different laws from the atom. It moves relatively slower. A cell is not only governed by chemical laws but it takes nourishment and discards matter, reproduces itself and feels, adjusts itself to the environment, inherits this adjustment—all of which the separate constituent elements are not able to do. The simple summation of the elements does not explain the laws of the cell's activity. It is an entirely new thing, a new whole and its laws are specific to this new whole as such.

It is not the intention to deny that the new entities originate upon the basis of the relationships of antecedents. Molecules originate from atoms combined in a special way etc. However, once the atoms are united in a molecule, they lose their independent atom-qualities and follow new laws. Unless it were so nothing new would originate in the world. Our task is to study this process. Let us take as an example the development of the individual cell. The cell develops by splitting. What splits? And into what does it split? A cell splits as a unity, its constit-

uent elements, the chemical matter, molecules, atoms, being only subordinate moments in the process. The cell splits into another cell, i.e. both new parts are again cells. The product is determined by the character of the original whole. It is a living thing, a living thing with history, with development behind it. The new cell is not simply a compound of chemical matter, it is the product of the manifestation of life, of the activity of the old cell as a unity.

In this manner the cells differentiate into diverse kinds and satisfy distinctive functions in the organism of a living being of higher order. The component parts of this higher organism are again cells, but differentiated cells, organs, which are created by the life process, by the inherent activity of the organism as a whole. Instead of the process of integration of lower elements according to their specific laws, we see the organism assuming the special form of activity of the living being, called the biological law.

Everything in the world (except the world as a whole) developed in this way by splitting from the continuum and not by an integration of originally independent parts. There is a fundamental difference between the antecedent factors, of a higher unit and the parts which the unit produces, into which it splits. The first are subordinate moments of the indivisible whole, which once created "jump" into a new order and obey new laws. It behaves not as an aggregate of the anticedents, but a qualitatively new whole which determines the specific character of its new parts. These parts differ from the whole, (are opposite to it in a certain sense) but are included in it at the same time and are determined by it. Without cell no animal organism, but without animal no differentiation of cells into organs. Without human beings no society, but without society no human beings.

It is not apparent to the eye that the human body is a complicated system of cells, but it is very obvious that society is composed of individuals. Bukharin remarks:

"Any object, a stone, a living thing, a human society, etc. may be considered as a whole consisting of parts (elements) related to each other; in other words, this whole may be regarded as a system." (Bukharin, N. I., *Historical Materialism*, p. 75.)

In human society these parts or elements are naturally human beings.

"Each system consists of its component parts (elements), united with each other in one way or another. Human society consists of people, the forests of trees and bushes; the pile of stones of the various stones; the herd of animals of the individual animals, etc." (*Ibid.*, p. 78.)

Within the society, of which they are component parts, men are related specifically by their production relationships. Bukharin describes it as follows:

"A consideration of the production relations will show that they depend on the manner in which the persons involved are distributed in space. The relation is expressed in the fact that each person has his place as a screw in the mechanism of a watch. It is precisely this definite situation in space, in the 'theater of labor' that makes of this arrangement, this distribution, a social relation of labor. No doubt every object is situated in space, but here men are joined, particularly by the definiteness of their working positions." (*Ibid.*, pp. 148–149.)

This definition reminds one closely of the mechanical world-pattern. The relationship of men is an arrangement in space. This geometrical arrangement determines social relationships. If so, why don't we call the relationships of material points, stones, trees and bushes "social relationships?" Because here *men* are in relationship. The particular feature in which the one differs from the other is the distinctive one. The constituent parts of things are supposedly material points, but their situation in space results once in a heap of stones, another time in a living creature, a third time in society.

To Bukharin men are not very different from material points. They are "active material bodies." Society is a "system of active elements," it is likened to a mechanism, a machine. Rudas assails this abstraction of every specific and qualitative feature, this reduction of human relationships to quantitative arrangements in space and quotes Karl Marx:

"If a thing is remote from another, the distance makes a certain relationship between the two; but the distance is something different

from this relationship. It is an expansion of space, a given longitude, which might be called just as well the distance between two other things as the ones we compare. But that is not all. If we speak of the distance as a relationship of two things we presume something special, an attribute of these things, which makes them capable of being distant from each other. What is the distance between the letter A and a table? The question is senseless. If we speak about the distance of two things, we speak of their difference in space. We presume that both are in space, both are parts of the space. We reduce them to 'Existenzen des Raumes'—beings in space—and after we equalized them sub specie spatii we distinguish them as different points of space. In their belonging to space is their unity." (Marx, *Theorien ueber den Mehrwert*, Bd. III, p. 170–171.)

Of course, if we abstract all qualities, if we disregard all other human relationships, if we reduce human beings, to mere "beings in space," in this case they differ only in their spatial arrangements.

How then did the "aggregate" society develop from this atomic spatial distribution? We saw already that Bukharin believes in the surrounding nature as the constant motive power for the development of human society. Was that enough to produce from the animal which was antecedent to man a being of higher order? And did this biological man produce in turn society by ranging himself into certain spatial relationships or patterns? Man, biological man is doubtless an antecedent to society; he is a basic factor, but not a constituent part in the sense in which Bukharin understands it. Because society is just as much an antecedent to man. Without man no society, but without society no man. The animal from which men developed was not yet a man. He arose in and from society, through the special production process which differentiated the animal-man into a distinctive social individual, the zoon politikon. Men are subject to certain biological laws, but society as a unit has its specific laws which are neither the sum, nor the continuation, nor the combination or modification of the old elements.

Bukharin following his mechanistic premises explains production relationships by a geometrical, spacial distribution of the individual in the production process. According to their place

in the process people can be grouped "joined particularly by the definiteness of their working positions." Marx on the contrary sees before all a qualitative difference between the classes. There are exploited and exploiter. Both are men, but in bourgeois society a general or a banker plays a brilliant rôle, a worker a miserable one. This qualitative difference can not be disregarded in a sociology which claims to be dialectical in its method.

Society is made of men, no doubt, but society is made of classes as well. Which are the integral parts of society, men or classes? Did men first unite into classes, then into society? Is society integrated from individuals, or did it produce its classes by "splitting," by differentiation? Rudas maintains that the origin of classes cannot be explained if, as point of departure, we take the apparent element, man. We have to proceed from society as a whole, which produced its parts according to the dialectical principle. It is a historically established fact that society was prior to classes. In prehistoric times production relations were not refined enough to crystallize such definite groupings.

The immanent movement of society, the inner contradictions which govern it are producing the differentiated parts into which it burgeons. These parts are new, they did not and could not exist before the whole, because their character is determined by the character of the whole. These organic parts are classes, not men. The mechanistic image of the whole which is nothing more than a quantitative aggregate is nowhere to be found in nature or society. The distinctive laws of motion of every new order produce their own inner contradictions which lead to further development. Bukharin does not see beyond the apparent elements in society, taken as an aggregate. By the reshuffling of these elements little new can be produced, and that is why Bukharin could not explain satisfactorily the existence of classes as organic parts of society. Only by ignoring his own premises can the mechanist achieve an eclectic unity between the social structure he professes and dialectical terminology and inferences.

III. THE CONCEPT OF CAUSALITY

"Until now, one hundred and fifty years after Hume, substance and causality still paralyze the daring of reason."

(Petzold: *Einfuhrung in die Philosophie der reinen Erfahrung.*, Vol. I, p. 31.)

"If twenty years ago, bourgeois philosophers, despite the spontaneous materialism of the majority of physicists, saw in radioactive disintegration of matter a proof of the disappearance of matter, and if further the theory of relativity has been taken as affirmative evidence of philosophical relativism, subjectivism, the bourgeois physicist of today resting their arguments upon the quantum theory, declare that causality has been overthrown and solemnly place upon the throne thus vacated the causa finalis of Aristotle." (*Science at the Crossroads*, Kolman, E., p. 83.)

The problem of causality has occupied the mind of philosophers and scientists since antiquity, but it comes more and more to the fore. This is a natural development. Causality was interesting to the scholastic mainly as a supplementary argument for the existence of God; it is an essential and central issue for the modern natural scientist. Our ideas reflect the social and economic structure in which we live, and the relative importance of specific theories varies according to historical conditions.

In the capitalist academic world the neo-kantian school of thought prevails. As Henry Poincaré expressed it:

"The laws of nature are symbols, conditioning limits which man creates for the sake of convenience." (*The Value of Science*, p. 13.)

We find a corresponding opinion in the writings of the Russian "renegade" Marxist, Bogdanov:

"the general law of the causal relation of phenomena is the last and the best product of human cognition; it is the universal law, the highest of those laws, which, to express it in the words of a philosopher (Kant, A. E.) human reason attributes to nature." (Fundamental elements, p. 42. Quoted by Lenin: *Materialism and Empirio-Criticism*, p. 136.)

We could present a wide range of quotations, from Avenarius to Karl Pearson, illustrating the subtle shades of this philosophical approach, as employed in the social and natural sciences.[12] Such

[12] "Necessity remains the degree of probability of expectation of the effects." (Avenarius, *Philosophie als Denken etc.*, paragr. 83.)

"The necessity lies in the world of conception and is only unconsciously and illogically

a methodological attitude degrades laws to something subjective, the product of mind. It fails to understand the relation between the general and the particular. The unique, the individual is emphasized, and the general becomes an illusion or a subjective symbol.

The historical economic-social basis is perfectly reflected in this arbitrary, individualistic method of approach to the sciences. The really important epistemological problem which divides the schools of thought, both on the understanding of scientific method and on political philosophy, is not concerned with the exactness of our description of causal connections, but whether the source of our cognition of these connections is objective reality (laws of nature) or the product of our reason. We can not enter here into the argument for the priority of matter or idea; we have presented the problem elsewhere. However, if we accept our experience as a more or less correct image of an outside reality, we arrive to the objective existence of the laws of nature.

In this case it becomes essential for science to decide, whether the specific law of causality is or is not an existing relationship, and if so, what exactly does it imply. Mechanics,—whether we accept its laws in their oversimplified form or not—served as a step forward in this direction, by focusing attention upon the problem.

Between indeterminism and mechanism there are many eclectic tendencies. The biological school especially—but physicists and sociologists do as well—challenges the conditionality of dynamic laws and introduce free, undetermined chance in the form of statistical law.[13] The change of words however, as Lenin says[14]

---

transferred to the world of perception." . . . "The necessity thus lies in the nature of the thinking being and not in the perceptions themselves; thus it is conceivably a product of the perceptive faculty." (Pearson, Karl, *Grammar of Science*, pp. 86–87, 179.)

[13] "Mises . . . proceeds from the principle that while dynamic laws govern the individual elements forming a manifold, statistical laws govern the manifold as a whole . . . statistical law is the law of the macrocosm, dynamical law the law of the microcosm." (Kolman, *Science on the Crossroads*, p. 90.)

[14] "We must note here that with remarkable naivete our Russian Machians change the question from the fundamental difference between the materialist and idealist arguments concerning causality, to the question of this or that formulation of the law. They follow the German empirio-critical professors, in believing that to say 'functional correlation,' the discovery of 'recent positivism' is to free us from the 'fetishism' of such expressions

does not change the thing in the least. It remains indeterminism and as such, subjective idealism, so that we have to search for other paths to arrive at the explanation of causation.

A. Determinism (mechanical)

The determinism, which was introduced by the French materialists, of the 18th century (on the basis of Newtonian mechanics) into modern natural sciences denies chance. Nothing can happen in nature and society without a cause, and the same causes will always produce the same effects. If that were not so, we would not be able to predict and get our bearings in nature. Life would become impossible. There is no accident; the term expresses only our ignorance of the laws which govern the appearance of the unexpected. Necessary means: causally determined; nothing more nor less.

"Strictly speaking . . . there are no accidental i.e. causeless phenomena." (Bukharin, *Ibid.*, p. 44.)

Hence, Bukharin means that everything causally determined is so ipso necessary, and there is no other necessity but causal. Using Engels' sarcasm: ". . . that a peashell has five peas instead of four, that a flea bit me last night at 4 a.m. and not at 5 a.m. these are facts, which are linked by an immutable chain of causes and effects; they are brought about by an iron necessity, and already the gaseous ball from which the solar system developed was planned so that these events had to take place as they did and not otherwise." (In Engels' *Naturdialektik.*)

Necessary is everything which has a cause; chance could be only a thing which has no cause. But because everything has a cause in the world, therefore everything is necessary.

". . . if, of two or more causal chains (series) of intersecting actions we know only one, the phenomenon obtained by their intersection will appear accidental to us, though in reality it is in accordance with law. (Bukharin, *Ibid.*, p. 44.)

---

as 'necessity,' 'law' and the like. Of course, this is absurd and Wundt had a perfect right to ridicule such change of words (*Phil. Studien*, pp. 383, 388) which does not change things in the least." (Lenin, *Materialism and Empirio-Criticism*, p. 128.)

Consequently to Bukharin what we call chance is not objective reality independent from us. There is no chance happening in nature and society. We call accidental a relationship the origin of which is unknown to us. Chance is something subjective as opposed to the rigid determinism of nature.

This might be good formal logic, but it is undialectical. Since the subjective (here the accidental) is not relevant from the point of view of science, solely necessity is interesting to it. If we follow the line of this reasoning we reach the absurd conclusion that what we know is significant, what we do not know (chance) is of no account. Whatsoever we can classify under general laws is necessary, what we cannot pigeonhole is accidental. One step further and we shall say: what we can explain is scientific, what we can not explain has supernatural causes. The unexplainable, the hidden cause whatsoever we may call it: chance, fate or God is essentially the same. It means: I do not know and therefore it is not science.

This kind of sterile reasoning does not get us anywhere. It leaves the most important problems unclarified. The concept of order, of quality is lost. The bite of the flea and the laws of the solar system belong to the same order. The qualitative difference in the two events becomes something subjective if not mistaken. This kind of logic degrades subsequently into accidentalism, because if everything is equally necessary, nothing is *more necessary* and therefore everything is chance.

This point of view has a false perspective. Essential relationships and processes are overlooked. The unimportant with its overwhelming mass obscures the important, the qualitative is dissolved in the quantitative, and we are lost in the complexity of events. From the colorless mass we select the events which *appear to us* or which we desire to be significant. Important and unimportant, necessary or accidental—both are subjective.

Nature and society are disintegrated again. There is no law, it is man who brings lawfulness into the chaos. What a paradox! Can there be anything more deterministic from the outset as mechanistic materialism with its all-inclusive rigid causal law, and still it leads us straight to its opposite pole, to subjective idealism.

"In a word, all events from the most petty and insignificant to the most epochmaking events of our times are equally not accidental, are equally conditioned by causes i.e. are equally the result of causal necessity." (Bukharin, *Ibid.*, 46.)

If everything is equally necessary because determined, necessity is condemned to produce only accidental; the absolute necessity and the absolute chance submerge and transcend into the metaphysical.

B. The dialectical concept of necessary and accidental

> "The accidental has a cause, because it is accidental; it likewise has no cause because it is accidental; the accidental is necessary; necessity determines itself by the accidental, moreover this accidental is the absolute necessity." (Hegel, *Logic*, Part II, Reality.)

How then can we get an objective criterion of relevance? Experience can guide us only if the relative significance of an event which impresses itself upon us has an objective basis, i.e. apart from being determined by causes, phenomena are quantitatively differentiated. Nature shows a hierarchy of order in which causality does not operate in a dull uniform manner. An infinitely involved universal connection characterises objective reality, and any given concrete event is at the same time cause and effect, conditioned and unconditioned, necessary and accidental, according to the rôle which it plays in one or the other relationship. Rudas remarks that the hero fighting on the barricades[15] might accidentally scratch his ear and still remain a hero. He becomes a hero independently of the physical discomfort which makes him scratch—by social conditioning from the point of view of which the scratching is incidental. It remains such in spite of determinism and not merely subjectively so, because we do not know why our hero scratches; Bukharin knows that he has a rash, and it still remains accidental. Even if a rash would become a mass phenomenon—says Rudas—because of some contagion, it might

[15] ". . . man does not constitute an exception to all the rest of the world: whether he desires to scratch his ear, or accomplish heroic deeds, all his actions have their causes. . . ." (Bukharin, *Ibid.*, p. 36.)

not eventually be a social necessity. On the other hand infant mortality, for example, is a socially necessary phenomenon, because it is socially conditioned (besides by physical causes) by the very structure of the capitalist system.[16]

There has to be objective accident in nature; without it there can be no real necessity. To trace the causal chain of the individual accident however, would be of little use to science. What science needs is to discover the law according to which these accidents change into necessity of a certain order. Engels presents a good illustration from Darwin's theory of natural selection. He says:

"Darwin in his epoch-making work proceeds from an exceedingly broad basis, resting upon accident. Precisely the unnoticeable, occasional difference in individuals of separate species, differences that may at length amount to a change in the very nature of the species, the most proximate cause of which may be shown only in very rare cases—precisely these facts compelled him to doubt the former basis of any law in biology, doubt the concept of species, its former metaphysical unchangeableness and permanence." (Engels, Notizen, *Marx-Engels-Archiv*, Vol. II, p. 266 ff.)

Variation may be considered as an expression of the accidental. It might be defined as something which is not directly related to the lawful inner development of a given phenomenon. But it might be an integral part, a cause of the process of becoming. The amazing words of Hegel, which were ignored by science and philosophy as verbosity come into their own. The individual's development has a cause, but it is accidental as long as it does not participate in the general (and precisely that which is individual in it has no significance for the general). If it transcends its own individual limits and so becomes the basis for mass-phenomena, e.g. for the development of a new kind, it becomes necessary,

[16] The query is often made why Marx selects the economic process "arbitrarily" from a vast complexity of events in which the economic phenomena have infinite non-economical implications. The theory of historical materialism is the theory of the qualitative differences in the order of social events. It considers one of these, the economic process, as the basic one in which all other social processes have their roots and from which they differentiate.

significant. This however originates from the accidental individual changes, therefore the accidental is necessary; the necessary is determined by the accidental. In contrast to the mutually exclusive mechanical concepts of accidental and necessary, we have the dialectical concept in its multiple relationships.

Rudas uses as illustration, Karl Marx's analysis of "accidental value." Among the forms of value which Marx analyses in his Capital, there is one which he calls "accidental." Does he mean that this value has no cause? Evidently not. At another place Marx calls this value "individual." As long as this value appears in single instances and is inconsistent with the dominant laws of production, it is relatively accidental. 20 yards of linen bartered for a coat represent an individual valuation of the type which occurs mainly in an economic system where commodity production is unknown, where exchange is rare and is exercized usually by the entire community at fairs, not by individual citizens. If it occurs, it is accidental both as to the commodities exchanged and the acts of exchange. But if these single acts of exchange are repeated more frequently, they will be a potent disturbing factor of the existing economic system. Why? Because this single, accidental value presents a contradiction to the collective barter of the primitive community, a step toward commodity production. As long as it is an infrequent phenomenon, it is accidental, in spite of its causal determination. As it becomes general, it assumes a necessary character. Instead of the accidental individual value, a general value will appear, from which in turn, money develops. This is necessary for a fully developed commodity-economy. The capitalist system is not conceivable without money as a general measure of economic value. What was accidental formerly—individual—is now necessary and general.

At last Rudas asks the question whether it is legitimate for Bukharin to identify the dialectical method of historical analysis with the search for the conditioning factors of some given event. He takes as illustration a historical hypothesis which was presented both by Bukharin and Karl Marx, and which later was verified by experience.

Karl Marx, in his letter to Vera Sassulich, writes,

"I analyse the development of capitalist production as follows: The radical separation of the producer from the means of production is fundamental in the capitalist system . . . the expropriation of the agrarian producer is the basis of this development. Only in England was it executed in a radical manner . . . but all countries of Western Europe go through the same movement." (*Capital*, French edition, page 315.)

"The historical necessity (Fatalitaet) of this movement is also exclusively limited to the countries of Western Europe. Why this? This limitation is referred to in Chapter 32 as follows:

"The individually earned private property is supplanted by capitalist private property, which rests upon the exploitation of the formally free labor of others." (*Ibid.*, p. 340.)

"In this western-European movement we see the change of one form of private property to another form of private property.

"The analysis given in *Capital* is therefore not an argument for or against the vitality of the Russian-agrarian-communism. A special study, which I made on the basis of original sources, however, convinced me that this communism can be the point of departure of the social regeneration of Russia. But to have it function as such, many conditions which limit it from every side must be eliminated, and then the normal conditions for a spontaneous development should be secured." (*Marx-Engels Archive*, Vol. 1, p. 341 f., French edition.)

Marx uses in this letter both methods of analysis: the dialectical and the "search for conditioning factors"; the dialectical which explains the necessary trend of the development of private property and the finding of the conditioning factors which may produce the transformation of agrarian communism into a higher form of communism.

The contradictions inherent in private property drive this from a form of self-earned private property to another, that of the capitalist form of private property. The motive power of this development is the inherent dialectical contradiction of the institution of private property. Marx, therefore, does not question whether socialism will or will not be accomplished. It follows necessarily from the further development of the capitalist system, based on

private property, which created since its inception its dialectical negation, the propertyless class. The negation will be negated.

The conditions under which this historical necessity will be realized belong however to another order.

"To make world history would, of course, be quite convenient, if the struggle be undertaken only under conditions of infallibly favorable chances. On the other hand, history would have a very mystical character, if 'accidents' had played no part. These accidents enter, of course, by themselves as a component part in the general process of development, being outweighed by other accidents. But the acceleration and retardation depend to a considerable degree on these 'accidents' among which figure also such 'accidents' as the characters of the people who stand at the head of the movement." (K. Marx, *Letters to Kugelman*, French edition, No. 46, p. 164.)

The first attempt might be a failure (as it was in the case of the Hungarian and Bavarian Soviets). But neither the retarding nor the accelerating accidental circumstances change the historical necessity of socialism which develops from the inherent contradiction of the capitalist system.

In the case of the Russian village communism such a dialectical contradiction was not present. It might be the basis for a higher form of communism, if certain conditions existed or were brought about. The eventual change from village communism to capitalism was not the consequence of some inherent causes. It was due to external pressure. Therefore the disintegration of the Russian "mir" (the village communism) was not a historical necessity in the sense in which Marx uses the concept. It was however later achieved and Marx examines the conditions which further or retard this process. On the other hand the fact that the conditions which could have achieved the development of agrarian communism to a higher form of communism did not materialise do not change the historically determined fate of the accomplishment of communism in Russia. We should not forget that Marx's argument was made half a century before history verified his prediction.

Bukharin's method is different. He says: "If society develops further, socialism is inevitable." This means that the coming of

socialism is not determined but will depend on pragmatic conditions which might or might not be brought about. The achievement of socialism is in Bukharin's method in the same order as the fate of the "mir" is in Marx's thinking.

Mechanical determinism and indeterminism approach each other dangerously. This narrow minded interpretation of causality comes partly from the mistaken identification of that concept with the "law of nature" in general.

"How shall we explain this accident in terms of natural law, or in other words, where does causal necessity enter here . . . the phenomenon will appear accidental to us, though in reality it is in accordance with law." (Bukharin, *Ibid.*, 44.)

Accidental and law are opposed to each other, whereas they are not opposites at all. The accidental has its laws just as much as the necessary. Probability is nothing more than the mathematical expression of the law of accidents. Moreover causality is not the only law, nor the only legitimate relationship. We have logical, mathematical, aesthetic, (not to mention dialectical) relationships—and so do we have specific laws in the respective orders. Mechanics at the same time as it disintegrates the world, oversimplifies it too. It paints a picture, which besides being incorrect (unscientific), is extremely dull and unpleasant.

What Is Topology?

BY

PHILIP FRANKLIN

INTRODUCTION. Topology (sometimes called analysis situs) is the most general and most fundamental branch of geometry. Logically its study should precede that of other kinds of geometry. But mathematical knowledge, whether regarded as part of our cultural heritage or as the possession of an individual, does not come into being like a building, from a completed foundation to a limited superstructure, but rather grows like a tree with ever-deepening roots as well as ever-spreading branches. So, historically, systematic studies in topology lagged behind Euclid's elements some 2000 years and were unknown a century ago. There has, however, been considerable activity in this field by the present generation, and a number of modern treatments are available for the specialist.[1] In spite of its fundamental nature, the subject is difficult, and many of its problems remain unsolved even today. This difficulty makes it pedagogically advisable to avoid questions of topology germane to other branches of mathematics and there is a distinct scarcity of material[2] available even to the mathematician, whose main interest lies outside this field.

[1] Alexandroff, P. Einfachste Grundbegriffe der Topologie (Berlin, 1932).
Kerékjártó, B. v. Vorlesungen über Topologie (Berlin, 1923).
Lefschetz, S. Topology (New York, 1930).
Seifert, H. and Threlfall, W. Lehrbuch der Topologie (Leipzig, 1934).
Veblen, O. Analysis situs (New York, 1931).
Waerden, B. L. van der. Kombinatorische Topologie. Deutsch. Math. Vereinig., 39 (1930), p. 121.

[2] Hilbert, D. and Cohn-Vossen, S. Anschauliche Geometrie (Berlin, 1932), last chapter.

Some elementary topological ideas have made their appearance in modern physics, e.g. the closed spaces of relativity and the phase spaces of statistical mechanics. This makes it desirable to attempt an answer to the question "what is topology?", comprehensible to the mythical educated layman.

2. Geometry. A geometry deals with the properties of figures in some space. The space might be 1, a spherical surface; 2, a plane, or space of points located by two coordinates x, y; or 3, a space (not intuitively visualisable) of "points" located by four coordinates x, y, z, t.

Certain groups[3] of transformations of a part of the space into another part of the space are permitted, and figures carried into one another by a transformation of the group are considered *equivalent.* Thus, as examples of transformations in the spaces mentioned, we have 1, rigid displacements of the sphere; 2a) rigid displacements of the plane; 2b) similarity transformations—displacements together with changes of scale; 2c) projective transformations of the plane; 3a) orthogonal transformations—equations in x, y, z, t, analogous to those expressing rigid displacements in x, y of 2a; 3b) Lorentz transformations in x, y, z, t,—a modification of those of 3a.

The group used determines the type of property studied, the theorems derived and the branch of geometry. Thus in the cases cited: 1 includes properties of material, rigid spherical triangles, independent of their position on the sphere. 2a) includes properties of material, rigid plane figures as studied in Euclid. 2b) includes properties of plane figures preserved under a change of scale. 2c) includes properties of plane diagrams preserved under oblique projections (shadows)—as on an inclined screen. 3a) includes properties of "rigid" figures in four dimensional Euclidean geometry. 3b), properties of figures in the space time of special relativity.

One geometry may be included in another, if the transformations of the *latter* are special cases of those of the *former.* Thus,

[3] Without giving the complete technical meaning of group, we note the principal property is that a succession of admissible transformations yields an admissible transformation.

since the rigid displacements of 2a) are included in the similarity transformations of 2b), any theorem of the similarity geometry (2b) is a theorem of Euclidean geometry (2a).

3. Topology. The transformations admitted in topology are arbitrary continuous point-to-point transformations. Thus plane topology may be visualized by imagining our diagrams drawn on a sheet of dentist's rubber, when stretched to lie flat. Now let the rubber be stretched more or less in any part, distorting the figure. Any property of the figure persevering is a topological property. Thus, topologically, a square, triangle and a circle are equivalent figures, but each is distinct from a ring bounded by two concentric circles. Since all the transformations of 2a), 2b), 2c) are admissible topological transformations, each of these geometries includes all the theorems of plane topology.

4. Intrinsic Properties. Originally all geometrical figures were regarded as idealization of objects in our familiar three-space. Thus, spherical geometry was the study of the surface of a material sphere. But, to study abstractedly properties of spherical figures in their relations with one another, we may ignore all points except those on the sphere itself. And, instead of the sphere, any representation of its points will do, e.g., a geographical map of the North and South hemispheres on two circles. If such a map is drawn, and we measure distances, angles and areas at each point in accord with the rules by which the map was drawn, the map plus the rules gives a complete representation of the points on the sphere. We need the additional convention that a line meeting the boundary (equator) in the Northern hemisphere is to be continued across it into the circle for the Southern hemisphere. Since distances and angles are changed by distortion, they have no significance for topology, so that for the purpose of studying its topological properties, a sphere may be represented by two circles, with the convention that their boundaries match up (Fig. 1a). The shape need not be circles but may be taken, e.g., as squares (Fig. 1b). Or a single circle (Fig. 1c), or square (Fig. 1c) with a part of its boundary suitably matched on another part may be used.

The method used to represent a sphere may be used for other

surfaces. Thus the square (Fig. 2a) represents the anchor ring (shaped like an inner tube), (Fig. 2c), obtained from the cylinder (Fig. 2b) which results when the edges *A B A* are brought together. If a knot were tied in the cylinder before joining, as

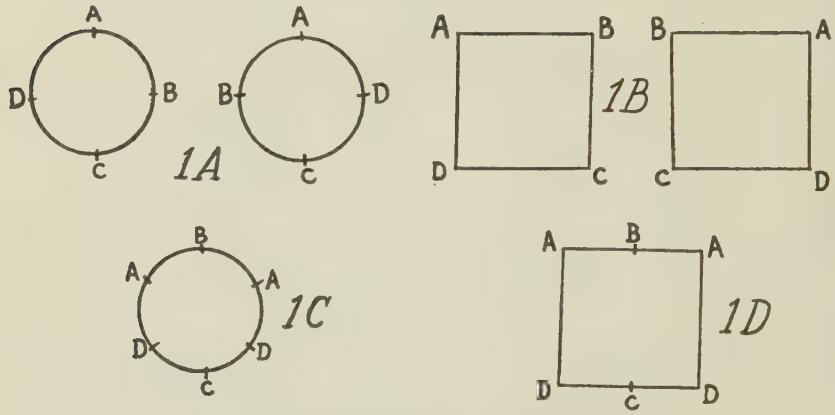


Fig. 1. Representations of the sphere

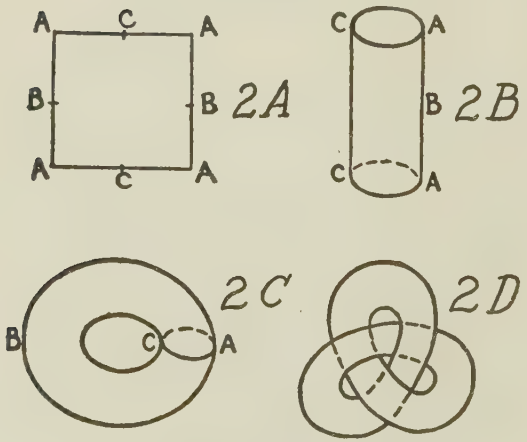


Fig. 2. Representations of the anchor ring

in (Fig. 2d), we should have a knotted anchor ring *not* equivalent to the anchor ring of (Fig. 2c) in the sense that a deformation of space could take one into the other. However, in an extended sense, namely, that they are decomposable into mutually equiva-

lent pieces, they are regarded as equivalent. Two figures equivalent in this extended sense are called *homeomorphic*.

5. Two-Dimensional Manifolds. Closed surfaces like those just described are called manifolds. They are roughly characterized as surfaces decomposable into a finite number of figures like circles or triangles, and by suitable decomposition any point may be made an interior point of one of the fundamental figures. A fundamental theorem (Euler's) states that in any subdivision the numbers of two dimensional elements, one dimensional elements and points A_2, A_1, A_0, are related so that

$$A_2 - A_1 + A_0 = K,$$

where K, the *characteristic* is the same for all subdivisions of a given manifold. Thus for the sphere $K = 2$, for the anchor ring $K = 0$, as may be checked from Figures 1 and 2.

6. Orientability. The sphere and anchor ring are *orientable* (two-sided) manifolds. That is, intuitionally, a bug crawling on the surface would never reach the same point on the opposite side. There are non-orientable (one-sided) manifolds. The *Möbius strip* (Fig. 3b) is a non-orientable figure. It is obtained by taking the strip of (Fig. 3a), and brings the ends together after a half-twist. A bug starting at P (Fig. 3b) and following the path $P\ R\ Q$, returns to the starting point on the opposite side. To describe the property intrinsically, we replace the bug by an index (sense of rotation) at a point, and find that a clock face, moving in the surface (Fig. 3a) along $P\ R\ Q$ returns with reversed sense. If the edges $A\ C\ B$ and $B\ D\ A$ of Fig. 3a are joined to the corresponding edges of the rectangle in Fig. 3c, a non-orientable manifold results.

7. Classification of Manifolds. Two dimensional manifolds are readily classified in terms of characteristic and orientability. It turns out that two manifolds are homeomorphic (equivalent in the extended sense) when and only when they have the same characteristic and are both orientable or both non-orientable. Orientable manifolds always have even values for K, the characteristic; non-orientable manifolds may have odd or even values for K.

Just as the anchor ring ($K = 0$) may be represented by the polygon of Fig. 2a of 4 sides, whose vertices correspond to the same point, any manifold except the sphere may be represented by a polygon of $(4 - 2K)$ sides. Representations in ordinary space like the anchor ring are also possible, which are smooth all over, but for non-orientable manifolds, the representations necessarily cut themselves.

8. Higher Dimensional Manifolds. Smooth figures decomposable into solid elements give rise to three dimensional manifolds. Thus, just as the two dimensional closed spherical surface could be represented by the two squares of Fig. 1b, so a three

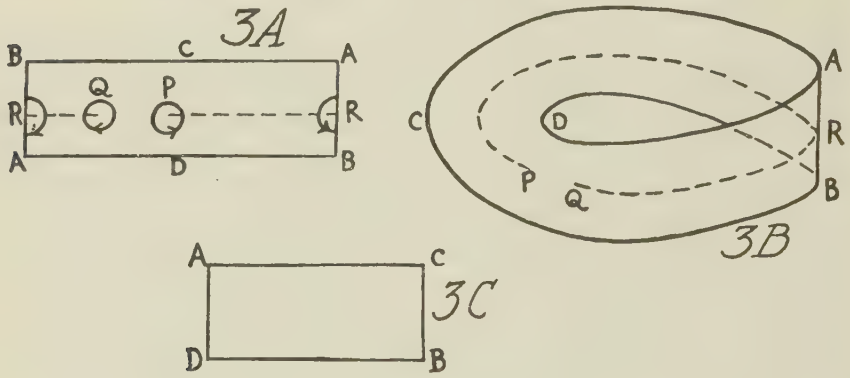


Fig. 3. One-sided surfaces

dimensional closed figure may be represented by the two cubes of Fig. 4, with faces matched as shown. We might imagine each cube a room, and a door cut in each face. A person in either room, stepping out the door, enters the other through the corresponding face. For him, nothing outside the interior points of the two rooms exists. This is one type of "closed universe" used by those who make cosmological speculations. While the evidence for any such property of our universe leaves much to be desired, there is nothing more impossible about the fundamental notion of a closed three-dimensional world then about that of a closed two-dimensional earth, which we have long accepted.

As a bit of mathematical whimsey, more suitable to the operations of a mathematical Ponzi, than to a legitimate cosmological

speculator, it is amusing to consider the possibility of a non-orientable closed universe. In such a universe, right-handed shoes would turn into left-handed ones on being transported around the proper path, and an individual who accompanied them would have his heart on the right side,—on alternate trips. Also any books taken with him would return with reversed characters, of the sort Alice met after going through the looking glass.

For higher manifolds, an analogue of the characteristic is known, as well as other types of number, and some properties only expressible in terms of the mathematical theory of groups. However, no method of classifying them completely is known.

9. Fixed Point Theorems. A large class of theorems of topology, of great interest in themselves and important for application

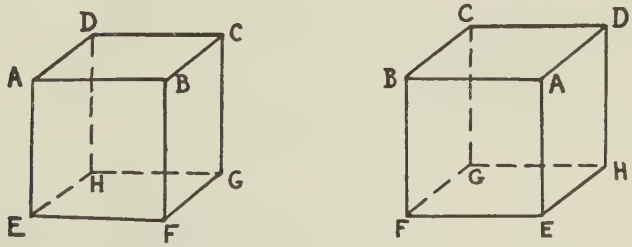


Fig. 4. Representation of a closed space

are those dealing with fixed points. As a first example, we mention the theorem that every continuous one to one transformation of a sphere into itself, which preserves sense of rotation, must have at least one fixed point. This theorem contains the real essence of the so-called fundamental theorem of algebra, which asserts that every algebraic equation has at least one (complex) root.

If we consider the orbits in a dynamical system, and begin with their intersection with a surface, we may follow the orbits around until they cut this surface again. This leads to a continuous transformation of the surface into itself, and a fixed point of such a transformation leads to a closed, or periodic orbit. This is the basis of the applications[4] of fixed point theorems in dynamics. These applications were responsible for Poincare's

[4] Birkhoff, G. D. Dynamical Systems (New York, 1927).

interest in topology and hence, indirectly for most of the later work in the field.

We add a theorem with an application of more personal interest, if less imposing than the applications to celestial mechanics. If, at each point in a circle or hemisphere a direction (small arrow) is assigned, we have a vector field. This is related to a small deformation, the arrows representing the velocities of the point at the beginning of the deformation. A theorem of Brouwer states that if all the points on the circumference of the circle or hemisphere move out (or all in) the deformation has at least one fixed point. In terms of the vector field, this means that if all the arrows on the boundary point out, there is at least one singular

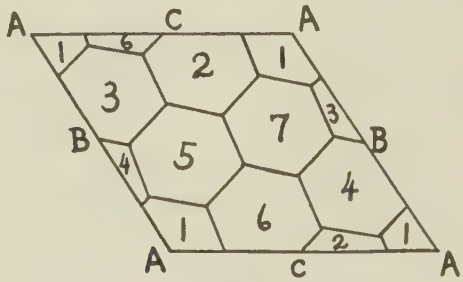


Fig. 5. Map on the anchor ring requiring seven colors

point in the field. This theorem is responsible for the swirl on the tops of our heads, which a biologist once thought a peculiarity of furred animals! Thus if there are Martians, and they have the same hair conformation at the sides, they must have swirls. If the circle is replaced by a ring, the theorem breaks down, as any bald-headed man can testify.

10. Basic Theorems of Space. As an example of those topological theorems which are fundamental to geometry, we may mention the theorem (Jordan's) that a closed curve divides a sphere into two regions. This theorem was illustrated by the drunk, wandering along the *outside* of a small fenced in enclosure, under the illusion that he was locked in. Topology does not distinguish the large outside from the small inside.

A theorem which throws light on the structure of space states

that spaces of different dimensions can not be mapped on one another in a one-to-one continuous manner. This theorem loses its validity if the word continuous be omitted.

11. Puzzles. Many puzzles[5] are essentially questions of topology, such as those dealing with labyrinths or the (known to be impossible) problem of joining three houses and three wells, each house to each well without any of the pipes crossing.

12. The Four-Color Problem. The question of coloring a geographical map so that no two countries with a common edge have the same color, with the fewest number of colors is perhaps the most famous puzzle of topology.[6] For the map on a plane or sphere, it is known that four colors are necessary, and that five suffice. Whether four suffice is unknown, and the question is interesting as being perhaps the simplest unsolved mathematical problem. Curiously enough, the problem is simpler on manifolds of other characteristics, the number being 6 for the manifold with $K = 1$, and 6 for the non-orientable manifold with $K = 0$, but 7 for the orientable manifold with $K = 0$. The map requiring 7 colors is given in Fig. 5.

Department of Mathematics,
Massachusetts Institute of Technology,
Cambridge, Mass.

[5] Ahrens, W. Mathematische Unterhalten und Spiele (Leipzig, 1918–21).

[6] Reynolds, C. N., Jr. On the problem of coloring maps in Four Colors, II. Annals of Mathematics, 28 (1927), p. 477–492.

Methodology of Modern Physics

BY

HENRY MARGENAU

I. SURVEY

METHODOLOGY might be understood to mean a description of various individual procedures which have led to the successful solution of specific problems. In studying the subject of physics from this point of view, i.e. with special emphasis on method, one would naturally turn his attention to the traditional divisions of experimental and theoretical physics, the former with its measuring devices and the latter with its mathematical technique. In no other sense than this does the term methodology make any direct appeal to the working physicist, and if you would ask him to define his methods he would probably answer with a description of experimental technique or the methods of setting up and solving differential equations. His answer would tell you *how* he solves his problems, but hardly how he *finds* them and *why* he solves them.

Nor does he assume this restrictive attitude from ignorance of the larger setting of his problems. His reasons for it are partly those of the artist who refrains from comment upon the origin of his artistic conception; they arise in part from his systematic tendency to put first things first, for he is afflicted with a keen realization of the futility of attempts to discuss scientific methodology in its fundamental implications before the incidental details of his technique are understood. If he is very conservative he may even maintain that his field is entirely foreign to extra-

physical speculations such as those necessarily involved in a basic evaluation of his technique, and that any mingling of his physical notions with such speculations debases the former. But he is more likely to admit the justice of philosophical circumspection, professing, however, neither interest nor competence with regard to the latter. Again, his lack of interest is well founded, for is it not true that the late brilliant scientific advances have been least coördinated with conventionally sound philosophic judgment?

Finally, there is some scientific modesty in the typical physicist's attitude toward "larger settings." In all his operations he prides himself on the accuracy of his judgments, he takes care not only to minimize his errors, but even to ascertain their probable magnitude. This he cannot do when he indulges in appreciative reflections upon the ultimate meaning of his work or upon its relation to the more speculative sciences. Hence he feels lost, being unable to use his customary tools, and he consequently avoids this awful territory.

In spite of this well founded preference of the typical physicist for his own technical domain, it is not the description of special scientific procedures to which this paper will be devoted. For, after all, there are cogent reasons, reasons indeed which should appeal to physicists, for surveying the special methods applied in any science from a vantage point which places them in view simultaneously. Unless this is done we shall be forever studying local currents and eddies in the larger stream composing our science, without discovering the general direction in which the water flows. True enough, we shall never know exactly where a boat, adrift on the stream of physics, will ultimately land, but we can, by taking a larger view and determining the main direction of flow, decide whether it will be worthwhile to set out at all. In less metaphorical language, what I shall try to undertake is to abstract from all specific and diverse physical procedures their common background of method, to analyze their residual features and to estimate their epistemological significance.

This task in itself is not new, and many attempts of carrying it through have been partially successful. I shall forego the opportunity of criticising them in detail and merely point to a de-

fect that has been very common. The diversity of valid physical theories is extreme, and it is easy in designing a general methodology either to forget entirely, or to underrate the importance of, apparently stray tendencies. It will be found that most writers, especially those whose main interest lies in philosophy, are using *mechanical* theories and laws as the sole basis for their abstractions, leaving out of account the non-mechanical ramifications of thermodynamics and electrodynamics. Indeed a methodology of physics is incomplete if it ignores the more formal reasoning which characterizes these two fields. In fact if we were permitted to limit ourselves to the experiments and theories dealing with large scale bodies our task would be a simple one. Difficulties arise, as we shall see, when we attempt to juxtapose highly formal methods and those perceptibly evident inferences which seem to govern large scale mechanics. It will also be necessary to *bridge*, not merely to contrast, the use of probability notions in some parts of physics with the idea of constrained evolution of mechanical states. These goals have not often been attempted, nor could they have been reached until fairly recently, for the very meaning of statistical theories, especially in connection with quantum mechanics, was not clearly understood; it seems to have crystallized only during the last few years. To bring the latest quantum mechanical developments within the range of a uniform methodology is perhaps the most difficult part of our task. It would be simpler if we could regard as a correct solution the interpretation of those who see in the new theories merely a rejection of causal description. But I believe that this extreme point of view, which on the surface seems to harmonize so well with the mathematical technique of the matrix theory, must be ultimately unsatisfactory and is due to an insufficient analysis of the basic concepts involved. It is necessary to admit that physical experience has in a sense become indeterminate, but the methods by which we describe physical states remain as causal as they were before. To put it crudely and in a manner which will receive its fuller meaning later, the world of physics has retained its causal consistency, while its connection with the perceptible world has been placed on a statistical footing. The recognition of the dichotomy

between the two worlds just mentioned is an essential feature of the doctrine which we are here proposing.

To meet more definitely the attitude of the staunch realist who sees no possible value in any extra-physical speculations about physical methodology one may appeal to the following consideration. Such an attitude can be based only on one or the other of the two suppositions: (1) The domain of physics is entirely secluded from all other fields of human interest, each field of thought being self-sufficient and independent of cross-fertilization. (2) Physics is the science *par excellence* and will, in time, pervade all other types of endeavor; the reason why we acknowledge a distinction at present is the imperfect state of analysis prevailing in the latter. Neither of these assertions can be said to be flatly false. But the first is clearly unsatisfactory in the sense that if it were to be ultimately true we should be sorry, for reason is unwilling to stop its inquiry in front of an array of pigeon-holes of which the contents are unconnected. If we maintain the second we are, I believe, indulging in an optimism for which there exists at present no sound basis. Perhaps the hope of such a possibility has vanished with the materialism of the last century. But if we wish it to be true, that is if we claim for physics the prestige of being the final touchstone of truth, then it is evident that we must enrich its structure by principles and methods not at present embodied in its constitution. We are safe in maintaining, therefore, that it is no disgrace for the physicist to speculate about the meaning of his science.

A methodology grows and changes with the expansion of knowledge. The impressive system of analysis outlined in Kant's "Critique of Pure Reason" was adequate for the science of his day, but his transcendental esthetics broke down when Einstein discovered the interrelatedness of time and space. More and more of his methodological principles crumbled away in the face of newly discovered facts. We are referring to this example because it shows so clearly how the individual tenets of a speculative system were invalidated by individual physical discoveries, a process which in general takes place more obscurely. Whatever claims may be made for any methodology of physics, finality can not be among its merits.

The question of method is very intimately bound up with the purpose or aim of scientific activity. It is only if we face squarely the somewhat annoying question: What is the physicist trying to do, that we come to grasp the nucleus of our problem. Let us, then, determine with some care the general object of the physicist's investigations.

II. DIFFICULTIES IN CURRENT CONCEPTIONS

It would be naïve to settle the matter by statements of the sort: The physicist explains nature, or he discovers reality. For in what sense does he explain? Does not such an utterance merely push the entire mystery of the physicist's activities into the word "explain" and seal them up, hidden from popular view? And then again: in what sense is his reality real? Does he not rather invent than discover? To such questions we desire to find an answer.

The object of physical investigation is not merely an accumulation of a certain type of knowledge, or of all the "facts regarding matter and energy," as elementary textbooks occasionally put it. The distinctive character of scientific systematization which pervades this knowledge does not exhaust itself in mere classification of facts. The existence of satisfactory physical theories which go beyond facts is sufficient proof for the inadequacy of the view that regards physics as a mere aggregation of knowledge.

But perhaps we are dealing with this argument in too summary a fashion. For there is, after all, a more painstaking group of analysts who will admit that a mere collection of facts does not make a science, but that aggregation must be supplemented by abstraction, synthesis and induction, processes which transform the collection into a logical array of facts. This claim, it must be observed, is usually made by those who wish to bar transcendental elements from the realm of physics, and this wish is, perhaps unwittingly, the father of the thought. Hence to answer this assertion in toto one must destroy two illusions: first that physics is based solely upon the logical principles of deduction and induction; second that it is unscientific to harbor transcendental notions, that is notions which do not spring directly from ex-

perience. We have already dealt with the latter thesis in the preceding chapter and shall meet with it again. All of modern physics compels this view. The question of the intrusion of intrinsically non-empirical concepts into our science has ceased to be an academic one; only by taking proper account of them can we understand the recent formalization that has taken place in physics. But this is a point to which we must return.

As to the contemporary structure of physics, even with the exclusion of the latest development which to some appear still slightly problematic and of doubtful security, this is certainly not a logical array of facts. For, though fact be taken in its widest possible meaning there can be no way of saying that the changing symbolism of atom clouds, electron waves, vacancies in the distribution of electrons can represent facts. I am not trying to belittle these physical notions; if anything, I am belittling facts. Facts are far too crude to fit into the delicate texture of physical theories; it is to mitigate their crudeness that the latter exist. It is true that logical principles, like deduction and induction, are used in physical as in all other reasoning, but their use is by no means characteristic. Induction, in particular, is used only as a tentative procedure; that is, the propositions to which it leads are always subjected to empirical verification and never proclaimed as certainties. For this reason, whilst the physicist employs inductive reasoning, he need not trouble himself with the profound problems involved in a logical analysis of induction.[1]

Next we come to the view, widely held at present, which identifies the object of physical investigation with the acquisition of facilities to predict. It can not be denied that physics provides such facilities in an ever increasing measure and that its practical value largely resides therein. But is not this advantage rather a by-product than the sole end of research? Scientific knowledge is power and will forever be power; nevertheless there are many who claim that they would pursue their scientific endeavors even if this were not true. They call their motive, somewhat enigmatically, love of research, much as though it were a natural in-

[1] As presented in the work of Mill, Jevons, Russell, Keynes.

stinct whose satisfaction gave them pleasure, and this pleasure is not peculiar to the act of prediction; it is present whenever a problem is solved. Among these, we believe, would be some of the most successful physicists. Thus it seems, at least from a psychological point of view, that the driving force in physical investigation can not be exclusively the fun or the profit of prediction.

Then, too, there are numerous physical theories which, thus far, have failed to make any noteworthy predictions. The theories of molecular binding and of ferromagnetism are among them. In these two fields experimental knowledge is so far ahead of "explanation" that a long time is likely to elapse before any significant purely theoretical predictions of new phenomena will be made. Yet both of the theories are regarded very important contributions despite their remote chance of predicting. The value of physical theories is clearly not fixed in proportion with this particular merit. Nor does a physical theory lose its value when its range of application is almost closed and it is hardly in a position to enable further predictions.

The misconception that scientific research be essentially a matter of predicting new phenomena has produced a most insidious practice not altogether uncommon among contemporary physicists. The scheme is to publish, from time to time, a sufficient number of predictions without giving detailed reasons for them, so that at least a few of them will come true by the laws of chance. Since valid predictions are very impressive and the memory of blunders tends to fade away, this scheme is sometimes regarded a vehicle to fame.

Evidently the physicist's business involves something more than prophecy, something that makes his experience peculiarly coherent and produces an internal fitness which facility for prediction alone does not convey. We know a great deal, for instance, about the phenomena attending the flow of an electric current through a wire at very low temperature and we have a strong desire to understand them. In spite of this experimental knowledge, in spite of the fact that we can predict many of the things which will happen when similar experiments are tried, it

is felt that this subject is in an unsatisfactory state, and physicists would be grateful for a (correct) theory which will do no more than "explain" these facts. The meaning of this term, explain, is the object we are seeking.

Is it the exposure of causal relationships? This is indeed frequently asserted. To decide the issue conclusively one must look more closely at the meaning of the word cause. An occurrence or event is said to be the cause of another if it precedes the latter invariably in time. Let us not worry too much at this point about the phrase "invariably," for we are not attempting here a critical discussion of the matter but only to find the customary use of the term. Moisture in the air, we say, is the cause of rainfall, or economic maladjustment the cause of war. It is to be noted that the causal relation connects two situations both of which are perceptible or, in a sense to be defined later, both are data. But this is perhaps the most primitive use of the word and clearly one that has little to do with physical research. As we have seen, the physicist does not satisfy himself with a record of facts, not even if they are placed in their correct temporal order. It is true, of course, that causal relationships of this type are the basis on which he prospers, but it is equally clear that he elevates himself far above them.

We can also say: A certain thought is the cause of a question; or a given mathematical condition is the cause of a special solution, without running any risk of being misunderstood. Here the word is employed in a different way, for the situations which are connected are no longer perceptible, and the time element is pushed slightly in the background. To be sure, the physicist often uses cause exactly in this way, as when he speaks of mass as causing a gravitational field, or of conditions causing constraints. In fact all imperceptible concepts occurring in physical theories are embedded in causal connections of this type, but to place concepts in such relations does not constitute a physical theory. Again we see that the statement "the object of physics is to set up causal relations" aims at the target but misses the mark. For, besides being related among themselves, these concepts must be in some way connected with experience.

Obviously now, there is a third manner of using the term cause, exemplified by the statement: an electromagnetic field of rapidly fluctuating intensity is the cause of the sensation of light, or the chair which I kicked has caused me pain. Here a relation is set up between something that is perceptible and something that is not. The time element has nearly dropped out of the picture and its place has been taken by a curious notion of enforcement. The use of the word has become very hazy indeed, as probably even those will admit who attach little weight to the difficulties attending the relation between stimulus and perception. Philosophers sometimes argue that this use of cause is unfortunate and should be abandoned. To this conclusion I should like to subscribe, not so much out of philosophical convictions, as because it is unwise to use one term in too many different ways. However, it is precisely this latter sense in which most advocates of "causal explanation" wish the word cause to be understood. They would suggest that physics supplies unobservable causes for the events of experience, thus rounding off the universe in a satisfactory fashion. Statements of this kind can not be spared the criticism of indiscrimanacy of speech.

But there is a more serious reflection upon this view. People who hold it are not always careful to keep the *notions* distinct. Thus, by using the term cause in the last sense, the force of gravity may be said to be the cause of a body's fall. By using it in the first one could say: the force of my arm (muscular exertion) is the cause of the stone's flight. In both cases the antecedent is a force, the consequent a type of motion, the relation is said to be causal. Nevertheless every physicist realizes that the term force in the last case is not understood in its true physical sense, but one of sensory perception. Thence has sprung the confusion in the meaning of force, and the resultant difficulty, known to every teacher of physics, of conveying to the beginning student a properly abstract and non-anthropomorphic conception of this physical quantity. Force is not something that can necessarily be described as a push or a pull.

Let us now, after finding such an immoderate amount of fault with supposedly current notions, set out to determine the object and the course of physical investigation.

III. PHYSICAL RESEARCH AS SYMBOLIC CONSTRUCTION

Physical inquiry in its essential form moves along a peculiar cycle: it starts with definite, perceptible matters of facts; proceeds from there into a field in which at least some of the elements of operation are not directly perceptible, where there is greater freedom from empirical constraints; and finally it emerges again in the realm of perceptible facts. To illustrate: we observe a falling body, or many different falling bodies; we then take the typical body into mental custody and endow it with the abstract properties expressed in the law of universal gravitation. It is now no longer the body which we originally perceived, for we have added properties which are neither immediately evident nor empirically necessary. If it be doubted that these properties are in a sense arbitrary we need merely recall the fact that there is an alternate, equally or even more successful physical theory—that of general relativity—which ascribes to the typical bodies the power of influencing the metric of space, i.e. entirely different properties from those expressed in Newton's law of gravitation. We are here clearly operating on a plane where there are fewer constraints than in the domain of perceptible facts. But having specified these properties, we at once return to the latter and say that the planets should revolve about the sun in elliptical orbits, or that the perihelion of mercury should advance through a specified angle per century. To be sure, a great amount of mathematical reasoning is involved in this simple return, but this represents only a formal expansion or modification of the properties produced in the former act of speculative creation. We say that our "explanation" of the original observation, the falling of bodies, is in error if the latter propositions are empirically false. In case of their verification it may be either correct or false. Our procedure begins and ends among matters capable of perception, the totality of which we shall call *nature* for the sake of definiteness.

Let us choose another example. The physicist observes, as part of nature, the deflections of an ammeter placed in an electric circuit. In particular does he notice the changes in the position of the pointer which take place as he introduces more or less wire

into the circuit. He then invents, in the privacy of his speculations, entities of which he has never made the acquaintance through his senses although he may feel that he has perceived "similar" things in other connections; these entities were named electric current, resistance, electromotive force. If you press him, however, he will admit that no stock is to be placed in this similarity aside from its helpfulness in visualization, and that these entities are characterized by the properties which are assumed for them, that is, the relations in which they can enter. In the present instance, one such relation is Ohm's Law. This, of course, does not exhaust their definitional properties, for they may also enter relationships with other constructs (heat generated, etc.) not here introduced. From Ohm's Law we can then predict other experiences, which, if verified, lend some support to this mode of explanation. It is important in this connection to see that the return trip to nature is made possible by placing the speculative elements themselves, i.e. current, resistance, electromotive force, in definite correspondence with experience. We must know how to identify in a quantitative way the experimental counterpart of a current, resistance or E.M.F. The physicist expresses this by saying that he "measures" current strength, resistance, or E.M.F.

The same phenomenon permits an alternative explanation. The physicist can make his notion of an electric current more definite by supposing it to be a stream of entities known as electrons. To these electrons he assigns properties such as being charged, or of producing certain effects when set in motion, effects which are observable only through their experimental consequences. The electrons themselves are never objects of perception despite the intriguing vividness of Wilson's cloud tracks or the moving oil drops in Millikan's famous experiment. They can not be said to constitute nature in the sense in which we wish to employ the term. (Cf. also §V.) But from the notion of electrons we can, by virtue of definite rules of operation, go back to nature and make specific statements about perceptible matters of fact.

There is not of necessity any conflict between these two ex-

planations of the same phenomenon. For we may satisfactorily assume as one of the properties of electrons that their motion constitutes a current of the former kind, thereby rendering the first explanation reducible to the latter. The electron theory of current flow has the semblance of greater concreteness and makes, perhaps, a more direct intuitive appeal. But, we repeat, the construct of an electron is just as remote from the source of physical inquiry, nature, as is the less easily imaginable notion of a current of electricity, with electricity intuitively undefined. This semblance of difference has led to the distinction between phenomenological explanation—the type which satisfies itself with constructs of little or no imaginative appeal—and, causal explanation, such as the electron theory. It is clear from the foregoing considerations that the use of the latter term is very much to be discouraged, and that, on the whole, the distinction is illusory. This last point, however, is one to which I shall return later.

The full course of physical explanation, as we have now seen, begins in the range of perceptible awareness, swings over into what we shall now term the field of *symbolic construction*, and returns to perceptible awareness, or, as we have said, nature. The field of symbolic construction is populated with many entities, such as masses, electrons, electromotive forces, which we shall call *constructs* of explanation, or, more briefly, constructs. These will be subjected to closer scrutiny as we proceed. The essential feature of a physical explanation is evidently the transition from nature to the realm of constructs, and the reverse. These transitions are governed by rules of which we must gain a clearer conception. But before we consider the relation between these two classes of concepts it is well to direct attention to several points which can easily give rise to misunderstanding.

I have not been very specific in the exposition of the meaning of "nature." The term is obviously not used with its customary significance. We have defined it as the totality of all matters capable of sensory perception, including thereby hallucinations as well as bona fide sense impressions. The former are, of course, of no interest to the physicist, and we shall suppose that there

are means of eliminating them from the remainder of nature. Again, not all the rest of perceptible situations have hitherto become the starting point of physical explanations, but they must be regarded as holding such potentiality for the future.

But now arises the question, so vexing to many philosophers: Are we going to deal with nature merely as a complex of awareness, or must we attribute to it the status of transcendental objectivity? It is very fortunate, I feel, that physics is not forced to make this decision. In fact we can, and shall in these considerations, pursue the most cautious and possibly the wisest, course of regarding nature to be *merely* the aggregate of our perceptible experiences. I confess to the belief that the reader will not doubt the existence of such an aggregate in his own mind, and that this aggregate is largely similar to that which I have accumulated myself. If I am wrong in this supposition there is nothing further to say, for physics, as well as any other science, is founded upon this miracle. On the other hand, if any one wishes to go farther, as most physicists do, and postulate an objective nature he is free to do so at his own risk; no science can compel this step. This paper is written from the point of view of one who is unwilling to sacrifice a grain of certainty for a pound of common sense evidence. Accordingly, we should replace the common term data, which occupies so prominent a place in the physicist's vocabulary, by *habita*, implying that there may exist no external agency to which we are indebted for its gifts. (I will add that on my part this attitude is purely academic.) But to avoid the coining of a new and strange term we shall continue to speak of the elements of nature from which the reasoning of physics starts, as physical data.

Every datum has the attributes of extension and duration. To determine the precise logical sense in which space and time may be said to be attributes of nature is a problem which need not be solved for the present purposes; the only point to be observed is that they are very essential attributes, so essential in fact that a datum not in time and space not only fails to be realized but is even unimaginable. It is for this reason that all constructs, which, as we have seen, must be accessible from physical data

(in a manner to be discussed in §IV), partake of temporal and spatial qualities in order to provide a possibility for correspondence with nature.

It is sometimes asserted[2] that all physical data are pointer readings. A statement of this sort is likely to emphasize the quantitative precision which the physicist enforces within his nature—there is no difficulty in the fact that we can arbitrarily enlarge and modify our perceptible experience—and this emphasis is certainly well placed. But as a statement of fact it seems in error. While pointer readings form by far the largest class of physical data, there are at least two others. The first comprises experimental decisions between two alternatives: Does a certain phenomenon occur or not? The very appearance or absence of a band in the spectrum of a given molecule may be a perfectly significant datum. The other is that group of experiments in which the investigator relies on counting individual events, e.g. the clicks of a relay attached to a Geiger counter. Many of the data of nuclear physics, recently acquired, are of this type. Thus pointer readings, while of very great importance among physical data, possess no peculiar distinction and do not exhaust the possibilities. This remark is not idle, for no satisfactory methodology can be constructed on an oversimplified basis.

The physical concepts belonging to the class called nature, such as position, matter,[3] spatial coincidence or distinctness, motion, etc., present no greater difficulty of definition than do other properties of so-called concrete things. Some of them, e.g. position, are usually taken as primary concepts of which the meaning is intuitively evident, and the others, like spatial coincidence, may then be defined in terms of them. But a far greater complexity arises in connection with the other class of concepts which we have termed constructs of explanation. For it is often true that here a single construct admits of many different definitions, of which two are typical, and it is important that these be distinguished. To be specific, let us select a few illustrative constructs at random: mass, temperature, electron, magnetic field strength,

[2] E.g., by A. S. Eddington, The Nature of the Physical World.

[3] Matter is a datum, but mass is a construct in the sense here advocated.

magnetic field. Let us admit at the outset that no definition is an internally closed affair; it always presupposes certain terms to be understood or situations to be realized. We shall find this to be true when we attempt to define these constructs. Now then, how can we define mass? One possible procedure is to select a standard physical operation which can be performed upon every object to which we desire to ascribe a mass. We may then define mass as a definite functional relation between certain results of this standard procedure.[4] This definition is equivalent to the assignment of a perceptible, quantitative counter part to the symbolic construct "mass." It is, however, not unique, for there is a great number of processes which might be chosen as standards, e.g., gravitational interaction of two bodies and observation of accelerations, attachment of a body to an elastic spring and observation of the resulting acceleration, etc. Logically, each of these defines a *different* mass.

An alternative definition is available if, for example, we suppose the concepts of force and acceleration to be understood, for we can then say in general that mass is the ratio of force to acceleration (speaking non-relativistically, of course) without specifying a single operation. It is true that from a physical point of view this definition is equivalent to the former because it happens that the mass of a single body is independent of the choice of force, so that standardization does not detract from the generality of the definition. Indeed if this were not so the concept of mass defined in the present manner would be devoid of physical interest, but not of meaning.[5] Viewed logically, the definition of the present paragraph, which is not itself in terms of experimental operations (the auxiliary concepts involved may be thus defined!) and which I will term the *constitutive* definition, is different from that in the preceding paragraph, to be called the *epistemic* definition. Constitutive definitions are largely frowned upon by physicists, and epistemic ones are often considered the only admissible type. Nevertheless the interdependence of the

[4] See for instance E. Mach, Science of Mechanics; R. B. Lindsay, Physical Mechanics.

[5] This is contrary to the view expressed by P. W. Bridgman in The Logic of Modern Physics. See, however, H. Dotterer, Monist, 44, 231, 1934.

two types is plain, and the fact is, I think, that in virtue of the epistemic definitions of our constructs we can measure, and in virtue of the constitutive definitions we are enabled to reason about the constructs. Epistemic definitions alone would leave physical constructs without coherence, moreover, there are constructs which do not admit of this type of definition as we shall see. The present example is perhaps not very well adapted for exhibiting the necessity of constitutive definitions; the following one is more suitable to this end.

Let us look at the construct temperature. We are well acquainted with its epistemic definition in terms of the efficiencies of reversible heat engines (Kelvin scale) or of thermometer readings. In this case, an important constitutive definition happens to be on record also, namely the mathematical one which characterizes Gibbs' modulus of distribution. This modulus, the reader will probably recall, is essentially identical with temperature.

In the case of the electron we find the epistemic definition missing, since the electron is not a construct to be "measured."[6] Any definition by means of operations would fail to define it. It is not difficult, however, to define it constitutively by reference to other constructs of explanation, or by certain mathematical relations which describe its properties. Magnetic field strength again is capable of both definitions: If we accept the validity of Maxwell's equations we can describe it fully in terms of measurable accelerations of magnetic poles, as is customary; otherwise, if we grant independent meaning to all symbols except H in Maxwell's equations we can, if we desire, define H by reference to these equations. The last of the constructs listed, the magnetic field, can not be defined epistemically; we must designate it as the region of space surrounding a magnet or current, or in some equivalent way.

All constructs can be divided into two groups: Those which can be defined only constitutively (electron, magnetic field, atom, molecule, wave, photon, etc.), and those which are susceptible of both definitions (mass, temperature, field strength, force,

[6] N.B. We do not question the possibility of measuring its charge or its mass, etc.!

energy, etc.). Constructs belonging to the former group will be classed as *physical systems*, the others as *quantities*. Through their epistemic definitions all physical quantities are measurable; moreover, they acquire variability of amount. A combination of quantities with a system will be called a *state*. Later (§ VII) we shall examine more carefully the ways in which states are formed; the simplest possible way is clearly a direct assignment of a certain number of quantities, each of given amount, to a specified system. But, as we shall see, the combination may be less direct.

To summarize briefly with the use of the conventions now set up: For every set of data the physicist creates a set of constructs, in which systems are somehow combined with quantities, forming states. Both data and states are capable of temporal variation.

We must now consider by what rules, if any, the correlation between data and constructs is made.

IV. GENERAL REQUIREMENTS REGARDING CONSTRUCTS OF EXPLANATION

Physical explanation would be a useless game if there were no severe restrictions governing the association of constructs with perceptible situations. For a long time it had been supposed that all permissible constructs must be of the kind often described as mechanical models or their properties, but this view is now recognized as inadequate. The only apparent justification for it arises from a false interpretation of the significance of constructs, namely that which sees in them a composing part of some mysterious "reality." Our analysis has shown that they are different from what one should plausibly call nature; hence they need not satisfy the intuitive requirements of any fictitious reality. They may, in fact, be as abstract or insensible as we please (Cf. §VI).

A. But, while no restriction can be made as to their choice, their use is subject to very strong limitations. It is easy to find a set of constructs to go with a given set of data, but we require that there be a *permanent* and *extensive correspondence* between con-

structs and data. I hasten to illustrate the meaning of this unintelligible label. Let us choose two examples from extreme parts of physical theory, the first from ordinary mechanics, the second from quantum mechanics.

1. The initial set of data consists in the momentary perception of an apple in the process of falling to the ground. The corresponding group of constructs of explanation includes: two masses, those of earth and apple, each with properties described as mutual attraction according to the inverse square law. Many other combinations of constructs might be devised to correspond to the momentary set of data. But we now insist that the correspondence originally set up be maintained in time, in other words, that the changes in the state implied in the properties of our constructs correspond to the changes in the data by the same rules which defined the original correspondence. In this particular case, the rule of correspondence is that the measurable acceleration of the apple was to be proportional to $\frac{1}{R^2}$; we postulate that this shall be true at any later time. (This means, of course, that the acceleration shall be nearly constant, since R, the distance between center of earth and apple is practically constant.) Also, this correspondence shall hold whenever the apple is dropped again. This is what we mean when we postulate a permanent correspondence.—But we require more. We wish to apply the same rules, not only to every other apple, but even to every other body. Thus by assigning masses to the planets and then applying the same rules of correspondence we may arrive at Kepler's Laws, and we demand that the entirely different set of data, motion of planets, conform to our expectation. We have extended the range of applicability of our initial set of constructs by making it correspond to a wider range of nature. These matters are almost trivial; our recapitulation of them has the only purpose of showing how they fit into the scheme of a general methodology.

2. Let us pass to the second example. The group of data in question is the measurement of the frequency of a spectral absorption line. Its constructional counterpart is a multitude of atoms characterized by mathematical quantities such as Hamil-

tonian operators, definite assignments of degrees of freedom (number of electrons). Out of these we can build variable states (ψ-functions) which carry in themselves the rules of their temporal changes. This assignment of constructs is to be permanent in the sense that a) to every state there must correspond a perceptible situation,[7] and b) whenever the measurement is repeated the same group of constructs must be applicable. The correspondence in question has to satisfy the requirement of extensibility, which may be seen from the following example. The Hamiltonian operator is modified if the absorption takes place in a gas under pressure, for then, by the properties of the constructs under consideration, it acquires interaction terms. Their influence, again in accordance with the rules of correspondence peculiar to this system of explanation, is a definite change in the frequency of the spectral line which is actually observed. Many other instances of extension are imaginable, and most of them are experimentally verified.

There is no sense in which the correspondence with which we are dealing can be said to be a one-to-one-correspondence. For in the first place, the smallest part of nature is not a datum, but a collection of data. An isolated element of perception, without relation and relata, is an absurdity. Secondly, every set of constructs contains some for which there exists no single perceptible counterpart as one may easily verify by scanning the foregoing examples.

As far as the permanence of the correspondence between constructs and data is concerned the matter is fairly clear, for whenever it fails the theory, i.e. the set of constructs, must be abandoned. The extensive character of the correspondence, however, presents a somewhat different aspect. It is not true that a theory

[7] This happens to be true; for if we allow the states of the constructs to develop under the influence of a light wave the distribution of frequencies in the spectral line changes in accordance with the initial rules of association between constructs and data. For instance, if the exposure to the light wave is short, the absorption line is broad; if it is long, the line is narrow. But it is important to observe that in this instance a construct[ional] state does not determine a datum directly, but a probability distribution among data. This, however, is not contrary to the principles outlined above, which do not require the rules of association to be point for point direct.

is discarded unless it is uniformly applicable to all parts of physical nature. The set of constructs used in the first example, the theory of gravitation, is not expected to correspond to all physical data; electrical charges, for instance, are denied the property of (gravitational) mass, and this procedure is simply an arbitrary means of stopping the original correspondence. We say that the theory has a limited range of application. (That we can define mass electromagnetically and thereby widen the range is true but incidental; we could have chosen a different, but probably less familiar example.) Nevertheless we feel that there are enough data which are "explained" by this theory to warrant its retention.

There is no exact criterion to fix the domain within which the constructs involved in any specific mode of explanation must correspond to experience. This indefiniteness is reflected in the difficulty of distinguishing a theory from an hypothesis. The latter is simply a theory which has, at present at least, a limited range of application. It is promoted to the status of a theory if and when its range is deemed sufficiently large to justify this more commendatory appellation; there is no sharp distinction.

It may be of interest to examine a few physical theories from this point of view. First, there is the type whose constructs, together with their relations to nature, can be extended to a coherent range of data, but fails curiously to explain a particular set of empirical facts. Lorentz' theory of the electron is an illustration. Its central construct was a small spherical aglomeration of charge furnished with a number of abstract properties, all correlated with definite data. This symbolism explained, for a time at least, all known experiences collected under the heading of electrodynamics, except that it gave no account of the stability of the electron itself, which is demanded by its rules of correlation. The theory was not rejected on this score, but it was felt to be defective. In fact, this defect has not been removed at present.

Secondly, there is the type of theory which can not be successfully extended for the opposite reason: instead of explaining too little it explains too much. Here Schrödinger's wave mechanics in its original form, that is, before it was supplemented by Pauli's

famous exclusion principle, is a suitable illustration. According to the rules of this theory, the states of individual systems such as atoms or electrons can be combined in any arbitrary manner (the resulting ψ-function may be symmetrical, antisymmetrical, or mixed). But no correlate on the part of nature could be found for some of these constructs. It was only by limiting drastically the permissible combinations (Pauli Principle) that correspondence with nature was restored.

Finally, it is possible for a single mode of explanation to fail on both of these counts. The early form of Dirac's theory of the electron, so successful in many respects, was guilty of this fault. It demanded the existence of negative kinetic energy electrons, constructs with properties so paradoxical that they could not possess an observable counterpart, and on the other hand it left the occurrence of electrons with positive charges unexplained Both these difficulties are now eliminated by a brilliant modification which we shall discuss later (Cf. § IX).

We have seen that in trying to extend the correspondence between a given set of constructs and a given set of data we often encounter obstacles of an essential kind. These have always been annoying to physicists and have prompted them to generalize or modify their constructs in a way that would avoid them. Great achievements have resulted from such efforts; long strides have been taken towards ultimate unification of the constructs of explanation. Indeed the successes, especially those attained in the general theory of relativity, are so sweeping as to suggest the question: Will it be possible to explain *all* of nature by means of one great symbolism, forming a hierarchy of constructs of which the basic ones are completely extensive without break? It is the hope and the inspiration of the theoretical physicist that it can be answered affirmatively, for this prospect alone makes his efforts in the deepest sense worthwhile. But like all ideals, it provides him with no assurance that it will ever be realized—we shall probably be forever approaching the object of our dream. Nevertheless there is fun in the approach.

B. The requirement of permanent and extensive correspondence between constructs and data has an important supplement,

namely that of *simplicity*. It demands that the number of unrelated constructs in any theory be small. Again, this requirement is not perfectly definite; but this lack of uniqueness is no lack of precision in our formulation of physical methodology: it corresponds to an actual indefiniteness in our choice of theories. There can be rival theories explaining the same range of data with equal success. If one involves as few constructs as the other it is difficult to decide between their merits. But faith in the ideal of an ultimate uniform explanation of nature then leads the physicist to assert that only one of the rivals can be tenable, and his usual procedure in these embarrassing circumstances is to wait for the emergence of new data which will encumber one theory with additional and unrelated constructs while merely extending the other. The former is then said to violate the requirement of simplicity and is abandoned on these grounds.

For a long time the caloric theory and the energetic theory of heat were competitors. The former assigns to the complex of data "hotness," the construct "caloric," which is to be understood as a large collection of extremely small and almost massless particles, much in the way in which the atomic or fluid construct corresponds to matter.[8] Thus the construct caloric is not entirely unrelated to others already in use; the quantities distinguishing it (compressibility, extension in space, etc.) i.e. the further constructs involved are familiar from the accepted explanation of matter; their number is small. Much the same can be said about the energetic theory, which assigns to hotness the familiar construct: kinetic energy of moving particles. The advantage of the latter was not at once apparent; we recall that Lord Kelvin was able to derive his absolute scale of temperatures on the basis of the caloric theory, Carnot explained his well known cycle and proved the theorem regarding the maximum efficiency of a reversible heat engine, and Clapeyron calculated without

[8] It is unfortunate, perhaps, that one must use such stilted language to convey ideas clearly. In simply saying: Caloric is a form of matter, we are guilty of logical confusion. The construct caloric is not the datum matter, nor a form of it. The trouble is that the word matter has a double meaning: 1) that of a datum, 2) that of a construct (collection of massive particles, fluid). Common speech, being developed on practical concerns, rarely realizes epistemological distinctions.

much error the value of the important constant (subject to a different interpretation) now known as the mechanical equivalent of heat, by using the caloric construct.

It was not until the experiments of Rumford and Davy were performed that a clear decision became possible. These experiments demonstrated that a body can become hot without having any matter added to it. In terms of the caloric theory this means that caloric can be created. Thus the construct assumes a property which sets it aside from the constructs by means of which we are accustomed to account for matter. If it is to be retained it must be counted as a *new* construct added to the others. In this respect the energetic theory is better; kinetic energy can be communicated without transferring matter, hence no further constructs need be introduced. Thus the caloric theory is exposed as inadequate because it violates the requirement of simplicity.

If we were at liberty to ignore this requirement the physicist's ideal of a uniform system of explanation could easily be attained. For we could populate the world of constructs with mechanical models capable of simulating all possible data. Poincaré, I believe, was the first to call attention to this fact, which can be demonstrated without trouble. But the number of independent constructs in this scheme would be so great that physicists would at once reject it. There are other difficulties in the use of mechanical models of which we shall speak later.

C. In part A of this section, when discussing the permanent character of the correspondence between constructs of explanation and data, we spoke of changes of state implied in the properties of constructs. State, we recall, is a purely constructional term and is not in general identical with a perceptible situation, although, of course, it must be capable of correspondence with data.[9] (Cf. remarks near end of § III.) In what manner are these changes of state implied? The answer is simple: They are enforced by our use of the *causal mode of description.* I do not wish to enter again upon a detailed discussion of the broadest

[9] Whether we regard states as a third class of constructs (beside systems and quantities) or whether we look upon them as associations of systems and quantities is a matter of practical indifference.

sense in which the causality principle is applied in physics.[10] The usual, though not the only possible way of satisfying the causal requirement in connection with the changes of constructional states is to subject the quantities whose changes are responsible for the modification of the states, to differential equations. The order of these equations in the independent variables, coördinates and time, depends in general upon the number[11] and the kind of quantities (i.e. constructs!) chosen to define a state. In mechanics, where the systems are particles and the associated quantities are positions and momenta, the equations are of the second order in the time. The same is true in a great number of other fields where the symbolism of explanation has been fashioned after the pattern of mechanics.

In general the physicist can *predict* as a result of the causal nexus between successive states. What he predicts is another state of his constructs, but from this new state he may proceed immediately to data using the specified rules of correspondence. In practice the process of prediction usually takes a standard course: One seeks a general solution of the differential equations, adjusts this solution to the known state at any given time t_1 and finally introduces t_2, the time at which the state is to be determined, in place of the variable t appearing in the solution. This state is then related to a complex of data in the same manner as was the old. Possibility of prediction of states is a correlate of causal description, but it is not synonymous with causality, as was pointed out previously.[12] It is clear, of course, that causal description also permits a retrograde determination of states in time, so that what has here been said about prediction is also true about reconstruction.

The reader should not overlook, however, that physical causality, which we have here isolated as one of the requirements for the constructs of explanation, represents an attribute of nature only in an indirect sense; for it signifies no more than that nature

[10] Cf. H. Margenau, Phil. of Science, 1, 133 (1934).

[11] Cf. for instance Poincaré's example of astronomers who can not observe the longitude of a planet. The Foundations of Science, p. 111 et seq.

[12] Ref. 10.

is so constituted as to be simply explicable, within the framework of the present methodology, by a causal symbolism. To say that nature is causal is superficial and ambiguous.

The requirements of permanent and extensive correspondence, simplicity, and causality exhaust the conditions to be imposed upon admissible constructs of explanation. The first and last of these partly overlap.

(*To be continued*)

The Biophysics of Space and Time

BY

N. RASHEVSKY

IN STUDYING various possible physico-chemical interpretations of biological phenomena we arrived by a systematical development of what we have called "Mathematical Biophysics" at the interpretation of even such complex phenomena, as those involved in learning, Gestalt-discrimination and Gestalt-transposition. Yet, in spite of the apparent progress made along this newly trodden road, a sophisticated mind may feel that we are advancing into a peculiar "cul-de-sac." Indeed we are discussing possible physico-chemical mechanisms underlying the discrimination, recognition and constancy-properties of various spatial forms. Yet we have not touched at all the much more fundamental question, which logically *precedes* all the above, namely, what can be the physico-chemical interpretation of the general and primordial fact that we perceive those forms as *spatial*? Can we conceive any physico-chemical model at all, which would illustrate how and why we experience everything as being in space?

At first the whole question put in this way may sound as sheer nonsense. We speak of a *physico-chemical* interpretation. But doesn't any such interpretation presuppose already a description in terms of space? Is any physics without space thinkable at all? Can and should it be the business of a biophysicist to concern himself at all with this question; should he not simply take space *as given* in his physical world and let the philosophers worry

about the metaphysical aspects of the problem, lest he should fall into a *petitio principii*?

Such an attitude may seem to be the most common sense from a physicist's point of view, who seeks to interpret some biological phenomena in terms of physical ones. But a closer examination reveals that such an attitude merely avoids the issue.

Our ideas about physical space and time have undergone in the last decades very radical modifications. Instead of two different things, space and time became but two different aspects of one and the same thing, and are considered as being physically qualitatively identical. As far back as 1909 H. Minkowski claimed, that "from now on space and time, considered separately, are reduced to shadows, and only their union possesses physical reality."[1] For a physicist we live in a four-dimensional world, and we are ourselves four-dimensional, "space-time" geometrical figures, of a "cylindroid" shape. This is not a mere "façon de parler," for space and time *are* identical in their nature and what would appear to possess for one observer only spatial extension, will for another involve also a time-coördinate, and vice versa.

Yet, even those of us who are thoroughly convinced of the ideas of relativity theory feel subjectively, that space and time *are different* things. We feel ourselves to be three-dimensional beings, living in a three-dimensional space, and changing in time. The physicist uses the four-dimensional world as his fundamental frame of reference, by means of which he describes physical phenomena. The "man in the street" uses as his fundamental frame of reference, by means of which he describes his crude observations, a three-dimensional space and a *qualitatively different* time. Were we to consider in our physico-chemical interpretation space as a fundamental in physics we would rather be expected to consider "as given" the four-dimensional world with its *qualitatively identical and interchangeable* four dimensions, instead of only shadowy space and time. But that would immediately lead to the difficulty that this fundamental thing, the four-dimensional world, which we take as given in our physico-chemical in

[1] Phys. Zeit. **10**, 104, 1909.

terpretation, is not at all "given" and "fundamental" in the thing which we seek to interpret. For psychologically we are given our ordinary three-dimensional space. We are thus of necessity led to the question: why is it that we, being in reality four-dimensional figures, do not *directly* experience this? And why, since the experienced number of dimensions does not correspond to the real one, do we feel our space to be three-dimensional, rather than two- or one-dimensional?

This question was one of the favorite topics of discussion of the late Henri Poincaré.[2] According to Poincaré, we experience our space to be three-dimensional because our bodies and sense organs are physically constituted in such a way, that our movements can *best* be represented by a three-dimensional group. Elaborating this idea further, it can be shown by considering imaginary beings whose psycho-physical constitution is radically different from ours, that indeed such fictitious beings should choose as their fundamental frame of reference, their "space," manifolds of a different number of dimensions.

But all that still does not solve our problem. For we are interpreting biological phenomena in physico-chemical terms. And if we hope that this interpretation will ever be logically complete, we will have to show, what the physico-chemical structure of organisms must be so that they would choose for their frames of reference our three-dimensional space and one-dimensional time. Furthermore this structure *must* itself be described in non-spatial and non-temporal terms. Otherwise we assume at the start what we seek to prove. We may even go further, and require of our interpretation to give us the reason, why we, as organisms, need any frame of reference at all?

But *do* we really need one? Is there anything like a frame of reference given in the objective world, or is the requirement of such a frame merely the result of our own psycho-physical constitution? The requirement of a frame of reference in mathe-

[2] Pourquoi l'Éspace a trois dimensions. Dernières Pensées. P. 55. E. Flammarion, Paris, 1920.
Science et Methode. P. 95 et seq. E. Flammarion, Paris, 1909.
La Science et l'Hypothèse. P. 68 et seq. E. Flammarion, Paris, 1918.

matical physics has largely grown out of the practice of analytical geometry, where some system of coördinates is made the basis of any description of geometrical figures. But classical geometry did not use any such frame of reference, and considered geometrical figures as such. Two decades ago vector-analysis was developed in such a manner as to eliminate frames of reference. Modern Relativity Theory, too, tries to eliminate particular frames of reference from its equations. It does this, however, in quite a different way, by giving them a "general covariant form."[3] Whatever method we adopt, there can be no argument about any inherent "correctness" or "wrongness." It is a matter of mathematical convenience. But we see that even simple, purely spatial geometrical relations can be represented without any *explicit* introduction of space as a frame of reference.

Consider now a physical object. In describing it a *physicist* will *first of all* give its coördinates in space and time. But usually this does not specify the object completely. It may emit radiations, and we must specify all the wave-lengths emitted. It has a definite mass, may have an electric charge, a magnetic moment; it has a definite temperature, odor, and what not. A non-scientifically minded person may pay practically no attention to the space and time coördinates of the object, but will concentrate his attention on its color or smell. So far as a mere description of the object is concerned, its coördinates are by no means any more important than its other qualities. As a matter of fact the spatial coördinates of the object may be perfectly legitimately considered as three of many other "qualities," which characterize the object. There is nothing outside of our subjective choice to give any preference to the "space-quality" of the object over all others. A creature without proprioceptive and kinaesthetic sense and with a very undeveloped sense of vision, but with a highly developed sense of smell would most likely choose by preference the "smell-quality" of the object. If it decided to refer *all* his observations to a frame of reference chosen, like our own space, amongst the many other qualities

[3] Cf. O. Veblens article: "Spinors." Science, **80**, 415, 1934.

of the object, it would use for such a frame of reference the smell-sensations. And since the latter form apparently a six-dimensional manifold, every smell being composed of six elementary smells, mixed in various proportions,[4] the frame of reference, the "space" of our fictitious creature would be six-dimensional. A creature with a preponderantly developed sense of color-perception would refer everything to a two-dimensional frame, in which a point is given by what we call the wave-length of light and by the corresponding intensity. To such imaginary creatures our physical world would look very different from what it looks to us. Their "physical picture of the Universe" and their physical theories would be equally different from ours. Thus, for instance, in our physical universe we may observe a luminous point emitting two different, monochromatic radiations. In the universe of the second of the above creatures this same phenomenon would be described as *two* points (in the color frame), having both a common "space-quality" (what *we* call *our* space). Conversely, what in our universe appears as two points, emitting both *the same monochromatic* radiation, would appear to be one point, possessing two different "space-qualities." A change in the difference of the emitted wave-lengths would correspond to a relative movement of the two points in the color-frame. What we describe as a luminous point emitting a continuous radiation between two given wave-lengths λ_1 and λ_2, would to our fictitious creature appear as possessing *extension* in his "color-frame." It would be a "body," with a uniform "space-quality" at all its points.

But still a mathematical description of the universe would be possible for any of our imaginary creatures. Even more, there will be a "point-to-point correspondence" between their description and ours. For we can write down a sort of dictionary which would "translate" any of our statements about the universe into theirs, just as we have done in the above examples.

Now that we have recognized the "relativity" of the choice of the frame of reference amongst the various qualities of the physi-

[4] H. Henning. Der Geruch. J. A. Barth, Leipzig, 1924.

cal objects, we may make one step further and describe the whole physical world *without any frame of reference at all.* Any object is represented by a set of points or an extensive "region" in a multidimensional manifold, formed by all the independent qualities, including space and time, necessary to characterize the object. We shall call it the φ-manifold. The number of dimensions n of the φ-manifold we do not specify. For all we know, it may turn out to be infinite. But it may well be finite. Whenever, as we try to do in physics, we reduce some qualities to others, we reduce the number n of dimensions of the φ-manifold. Thus temperature may not be considered as an independent dimension, being expressible in terms of others. The φ-manifold is also most likely anisotropic and non-homogeneous. If we admit that the physical qualities, known to modern atomic physics, are sufficient for a complete description of the universe, the φ-manifold may turn out to possess comparatively few dimensions. But at any rate, since besides space and time we have to consider in physics other things *in this space and time*, it is clear that n is greater than 4.

As we have seen above, what we call one point in *our* space may in general appear as two or more points in the φ-manifold. Therefore our own bodies with all their physical attributes will be in general represented by a set of either unconnected or multiply connected regions in the φ-manifold.

The problem of the physicist is then to describe as completely as possible the "geometry" of the φ-manifold. The problem of the biologist, or perhaps bio-physicist, is to describe the geometry of those regions of the φ-manifold, which are composed of points, considered to belong to the organism. All other points belong to the external world.

The first, most obvious property of the φ-manifold, that a physicist will find, is that it does not consist of isolated points, but rather of a set of lines, extending infinitely in two directions. This is what corresponds in our ordinary language to saying that everything happens in time, and that the latter extends indefinitely into the past and the future. We thus shall speak, exactly like the modern physicist does, of "world-lines" and "world-tubes" formed by bunches of world-lines. Only our φ-manifold,

to which we refer those world-lines, is more general, than the "world" of Minkowski or even of the General Relativity Theory. They are only sub-spaces in our φ-manifold.

To avoid misunderstandings, we must *very strongly emphasize* that the above is *not* intended to mean that theoretical physics must either now or in the future introduce this "frameless" method of description of the universe. Whether this shall ever happen or not, is entirely beside the point. Though the above outlined method of description is possible and quite legitimate, there is no reason at present to adopt it. But the reason we dwelt so long on this point is that we must for our present purpose emancipate ourselves, at least logically, completely from the spatial and temporal description of the organism, if we wish to discuss why of all other qualities those of space and time form our choice of predilection. Otherwise starting with the ordinary spatial description of the organism, we would only fall into a vicious circle.

Having thus emancipated ourselves from the ordinary spatio-temporal description of the organism, we may consider the question as to why from the n dimensions of the φ-manifold we have chosen four as being more fundamental to our perception of the universe and of those four we experience *three* as being qualitatively similar, whereas the fourth though of equal import is felt as "something different."

All what we have said about the physical structure and function of organisms in our previous writings[5] can, according to what we have seen above, be "translated" into our new language, which does not give any particular prominence to space or time. In particular we may consider changes in the organism, produced by a change of some of the φ-coördinates in the external surroundings. The latter changes we shall call "stimuli," the former—"responses."

To express ourselves more rigorously in terms of our new φ-manifold, we shall consider how changes along the world-line of an "external event" are connected with changes in the internal

[5] For references cf. our paper in Philosophy of Science **1**, 176, 1934.

"world-lines" or "world-tubes" of the organism. While in the inorganic world we shall find that in the majority of cases similar changes in the former "world-lines" are connected with similar changes in the latter "world-lines," we shall find for higher organisms, that similar variations along the external world-lines correspond in general to different variations in the internal world-lines. And if we attempt to represent mathematically the corresponding variations of the two sets of world-lines, we shall find, that the relation between the internal and external variations depend on the shapes of the whole parts of both sets of world-lines lying on one side of the points at which we consider the varia-

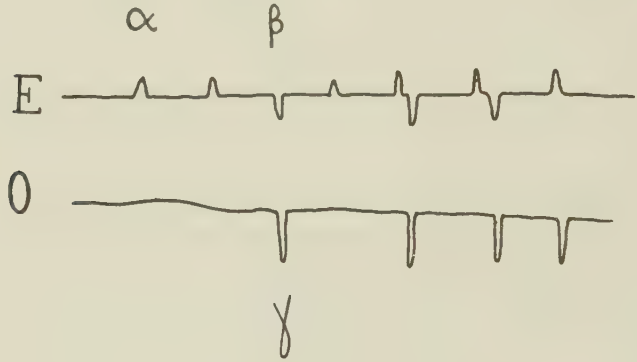


Fig. 1

tions. The mathematical relation thus formed will not be a differential equation, but an integro-differential one. This is what we express in our ordinary "space and time" language by saying, that the response of an organism to a given environmental change is determined by all the previous history of both the organism and the environment.

Thus in Fig. 1, if O represents the world-line of the organism and E that of some external φ-event, we see that to the change α in the latter does not correspond any change in the former. But a change β is connected with a change γ. This goes however only up to a certain point. When we find along the E-line an α-change succeeded by a β change, we notice that beyond that region of the φ-manifold, a γ change corresponds to an α-change.

This is a description in terms of the φ-language of a simple case of "conditioned reflex." Since we can build a system of physics in our φ-manifold, we can proceed also to interpret various biological and psychological phenomena in terms of physico-chemical changes. Only, using our φ-language we can make all our interpretation without reference to *our* ordinary space and time.

If the physico-chemical structure of the organism is such that, the law governing the connection of a variation in an external world-line with that of an internal would be the same for all kind of variations, no matter what φ-coördinates they involve, then of course no preference could be found in the response of the organism to the change of any particular φ-coördinate. But con-

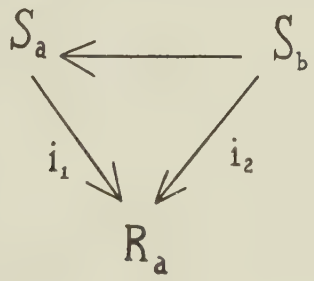


Fig. 2

sider different physico-chemical structures and in particular one of the following kind:

Let a response R_a always correspond to a stimulus S_a. But let S_b have, along some portion of the E-world-line, no corresponding response. Then let R_a become conditioned to S_b. S_b sets up a number of physico-chemical reactions in the organism, which in their turn, through various intermediate steps, elicit other physico-chemical reactions, which finally produce the response R_a. S_a sets up another set of physico-chemical reactions, which through *other* intermediate steps elicit R_a. The situation is represented diagrammatically in Fig. 2. We assume, that the intermediate reactions *are* different, and only result in a common end reaction. Now let the physico-chemical structure of the organism be such, that when such a type of connection between S_a

and R_a, and S_b and R_a is established, it results also in a physico-chemical connection between S_a and S_b of such an *irreversible* nature, that S_b will elicit a set of physico-chemical changes, which will produce the *intermediate* changes i_1 and result in the change eliciting R_a, but that S_a could not produce the intermediate changes i_2. So that while S_a elicits R_a only via i_1, S_b elicits it both via i_1 and i_2.

Inasmuch as we attempt to give a physico-chemical interpretation of biological phenomena, including those of psychology, we *must* postulate that whenever we speak of identical feelings or sensations, identical physico-chemical processes in our organism must correspond to those. There will be therefore a similar psychological element in the experiencing of S_a and S_b, since both produce i_1, but they will not be *equivalent* because of the above mentioned irreversibility of the processes involved. The intermediate physico-chemical reactions i_1 play a more important rôle than i_2. For a destruction or abolition of i_2 will not interfere with either the sequence $S_a \rightarrow R_a$ or $S_b \rightarrow R_a$. But the abolition of i_1 abolishes the connection $S_a \rightarrow R_a$. Psychologically the connection i_1 ($S_a \rightarrow R_a$) will be felt as a more fundamental one than the connection i_2 ($S_b \rightarrow R_a$), and the whole of the above-described physico-chemical set of reactions may be described in "antropopsychical" terms thus: the organism reduces or "refers" the changes S_b to those S_a which assume a rôle of a "frame of reference."

Without making any special hypothesis as to the detailed physico-chemical mechanism connecting stimuli and responses, we thus find a general formal structure, which those mechanisms must possess in order that from all the n φ-coördinates of the physical φ-manifold a number of them would acquire a psychological preference, and be used as a frame of "reference" for others. And since we actually observe that the "space-quality" and the "time-quality" possess this particular distinction, we may infer, that the actual physico-chemical mechanisms of our brain, regardless to their ultimate physical origin, do possess the above mentioned formal structure. It must be emphasized, that this formal structure does not imply any spatial relations at all. The

diagram in Fig. 2 is only a graphical isomorphous representation of this structure and need not be taken literally. To make the point clear, let us give an illustration.

S_a may result in the production of a series of chemical substances, *a*, *b*, *c*, *d*, *e*, *f* by a chain reaction. The last one *f* causes R_a. All those substances may be mixed and thus *spatially* not distinct at all. S_b may produce another chain-reaction: *g*, *h*, *k*, *l*, *m*, *f* ending in the same end-product *f*. Furthermore there may be an *irreversible* reaction: g → b. This purely chemical mechanism possesses the necessary *formal* structure. No need to say, that the above is intended *only as an illustration* and does not suggest any hypotheses as to the actual nature of the brain-mechanisms. But any such mechanism proposed will have to reckon with our formal requirements.

There is another circumstance which may give to some of the φ-coördinates a position distinct from that of the others. If we consider a world-line of the φ-manifold belonging to the organism, we find, that some of the φ-coördinates of points on this line can be changed by physico-chemical reactions, which take place only in the organism itself. To put it more precisely, the geometry of some of the sub-spaces within the organism is a function of the geometrical arrangement of internal world-lines only. In our ordinary language we describe this by saying that the organism can produce those changes spontaneously. And we notice in particular that the three dimensions of the "space-quality" have that property. This no doubt is a very important factor in the "selection" of a frame of reference. But it cannot be *the* critical factor. For the time-quality of an φ-manifold does not possess that property, or at least possesses it to a very small extent, as expressed by "memory." Yet time is one of our frames of reference.

We may however continue our inquiry further, and ask the following question. If, out of n dimensions of the φ-manifold, $m < n$ are chosen as a frame of reference, what is the physico-chemical structure of the organism, that determines whether it shall be an m-dimensional frame or whether it shall "split" into several "sub-frames," of corresponding dimensions m_1, m_2, ...

m_k, with $m_1 + m_2 + \ldots + m_k = m$? Such a splitting takes place in our own case, since we experience as two different things the one-dimensional time and the three-dimensional space.

The answer to the above question is again connected to our postulate that similar sensations and feelings must correspond to qualitatively identical physico-chemical changes. If the changes in the φ-coördinates $\varphi_1, \varphi_2, \varphi_3 \ldots \varphi_m$ produce all different physico-chemical processes, each of those φ_i may possess by virtue of the above discussed structure a psychological importance of a "frame of reference," but they all will be experienced as "different" things and form m separate frames. If however, besides eliciting each its own physico-chemical reaction, they *all* elicit one common physico-chemical process, things will be different. Let the intensity I of this process be given by

$$I = f(\Delta\varphi_1, \Delta\varphi_2, \Delta\varphi_3 \cdots \Delta\varphi_m) \qquad (1)$$

where $\Delta\varphi$ denotes the variation of φ, and f is a continuous function, arbitrary in all other respects, but symmetrical in the m variables. Then, due to the differences in the individual reactions, there will be a certain difference experienced in the different $\Delta\varphi_1$. But due to (1) they will also be experienced as qualitatively equivalent coördinates in an m-dimensional continum. The form of f in (1) will determine the metrical properties of that continuum, expressing the "distance" I in terms of the coördinates $\Delta\varphi_i$. If relations similar to (1) hold only between groups of $m_1, m_2 \ldots m_k$ of the $\Delta\varphi_1$'s, then we have k frames of reference, experienced as different things.

In the higher organisms the physico-chemical changes, connected with our time-sense, must develop in the very earliest stages of ontogenetic development. This is not surprising, for our sense of time apparently is caused by the very fact, that irreversible reactions take place in our organism.[6] Of all other sensations the proprioceptive and kinaesthetic connected with the spatial movements of our bodies, are the earliest to develop. If the mechanisms of responses to all other sensations are connected

[6] H. Hoagland. Jour. Gen. Psych. **9**, 267, 1933.

formally in the above discussed way to the mechanisms of space and time perception, then the time-quality and the space-quality of the objects in the universe around us acquire the preëminence of frames of reference. But we have two distinct frames, space and time. In connection with the above discussion, we must conclude, that there is no common reaction elicited by the two sets of qualities. Though we subjectively feel a relative difference between "height," "depth," and "width," (otherwise we would not have coined three separate words for them), we feel yet that they are of a qualitatively similar origin. A relation of the type (1) must hold for them. Time stands separate, not being connected to a "common" reaction. A mechanism may however be conceived, in which a change in a time-coördinate will produce a qualitatively similar physico-chemical change, as the change in any spatial coördinate. We must leave the description of such mechanisms for another occasion. Suffice it to say, that in our usual neurological terminology, it would amount to the assumption of a special "center," superposed on the centers, connected with the perception of space and time. If ever in geological ages to come, creatures will develop, possessing such a center, the relativity theory will be easier for their babies to grasp than the pythagorean theorem is to some "grown-ups" of our time.

Department of Physiology,
University of Chicago.

The System of the Sciences and the Organization of Knowledge

BY

HENRY EVELYN BLISS

I. SOME PRELIMINARY DEFINITIONS

IN THE survey and criticism of science and philosophy we are considering systems of knowledge and thought rationally developed from coherent human experience. These rational systems are in a definite sense intellectually organized. Where intellectual and social problems are real and urgent the relevant knowledge and thought should be organized socially and purposively to achieve tentative, if not adequate or definitive solutions.

Knowledge and thought, however distinct, are inherently interwoven in mental development and organization. Thoughts relate data of knowledge or experience. Mental organizations of knowledge may serve relevant organizations of thought and purpose. Moreover they may be intellectually purposive.

The organization of knowledge and thought is both mental and intellectual, that is, both psychological and logical. *Mentally* knowledge develops from coherent experience of actualities, learning of data, and memory of events; from perception and recognition of like characters, from measure and analysis of components, and also from apprehension of meanings, relations, and functions. *Intellectually* thoughts pass from concepts and terms

to judgments, opinions, and propositions. From logical inferences and intelligent comprehension of relations develop consistent reasons and the rational and coherent systems of science and philosophy. Organizations of Knowledge thus become organizations for thought.

Learning, knowing, and thinking as processes are subjective in individual experience; but these processes may also be objectively regarded by the individual subjects. Moreover the knowledge of realities, of their characters, constituents, properties, and relations, may be communicated to other subjects and organized in a community of minds by processes of abstraction and generalization somewhat analogous to those of individual minds. The resulting composite conceptual systems, ideas, and purposes may for convenience be termed *communal*, or social. They may be expressed in symbolic language or otherwise recorded, and thus they may be further objectified. This is done in descriptive, mathematical, historical, and critical study. Such communal embodiments of knowledge and thought are indeed distinct from the individual knowers and thinkers who originated or transmitted them, and they may be distinctly objectified.

These organizations are both "structural" and "functional," analogously and correlatively. The structures, or "patterns," or *Gestalten*, may be complex of component systems, which to more penetrating analysis are composite of constituent parts, or elements, and constitutive relations, which are external to the parts but internal to the systems. Structural social organizations of knowledge may become functional in relevant social organizations of thought and purpose. Some may be intrinsically purposive, or teleologic.

Organization is the comprehensive term, implying both structure and function. *System* connotes relations that are essential and constitutive, but they need not be functional. In *classification* the relations are merely implied. For precision we often write "system of classification" to distinguish this from a table, or *schedule*, of classification.

The correlative "polar" terms *object* and *subject*, *physical* and *mental*, *real* and *conceptual*, *structural* and *functional* have been

so confused in philosophic and literary usage that, if space allowed, we should define them here more precisely.[1] The term "object-matter" has been proposed for the correlative field of real objects studied in subject-matter, or, if not real, for objectified subject-matter; and it will be adopted here where appropriate. Thus, the object-matter of plants, their structures, functions, kinds, relations, products, etc., is subject-matter of botanical studies. The object-matter is physical, structural, organic, morphologic, classificatory, and mostly real. The subject-matter is conceptual, generally communal, and in some manner organized.

Classifications pertain to object-matter and inhere in subject-matter. Logically these two categories of classification are correlative and should be consistent. This is requisite for science and for common sense. In such consistency the order of nature subsists, and on it depend the value of study and discussion, prediction and verification.

All classifications, whether of subject-matter (conceptual classes) or of object-matter (real classes), are *relative* to subjects, to viewpoints, and to selection of characteristics or of principles; they should also be relative to the natural order of realities, so far as this may be discernible. Where they correlate logical subject-matters to natural object-matters, the classifications are *natural*, logical, or scientific. Otherwise they are *artificial*, arbitrary, or metaphysical; and such we need not consider here.

The order of a classification may be objectified in an arrangement of terms and symbols that denote the subject-matters, or object-matters. Such arrangements are usually referred to as tables, or *schedules*. The classifications are structural as distinguished from functional organizations.

We may now proceed to state the principles of classification for organization of knowledge and thought. The first is that of *organization* and unification *per se*. Division and partition may otherwise result in separation and confusion, where subject-matters are traversed by inconsistent interests and conflicting

[1] The interested reader may find the writer's definitions in his book on *The Organization of Knowledge and the System of the Sciences*, published in 1929.

purposes. Free thought, like free life, without organization, may rove and rave like Bedlam in a Tower of Babel. Tendencies to such confusions are evident at times—at present, one thinks—in philosophic, scientific, educational, economic, and bibliographic systems.

The second basic principle is the logical one of *subordination* of the more special classes, or terms, to the more general. Each sub-class is more specific than the more comprehensive class from which it is derived by division and specification. The third structural principle, that classes, or sub-classes, of the same order, or grade, may be arranged serially either vertically or horizontally, is usually termed *coördination*. It may be regarded as a corollary of the principle of subordination, or as complementary to it. Coördinate classes are serial as progressive steps on a level grade, or on a vertical ladder, or in a column. Subordinate classes are serial, or *scalar*, as successive steps in specification, to narrower extension, as when a ladder leans against a tree. A scalar series of classes, subordinate in one aspect (logically and vertically), may be coördinate in another aspect (tabularly and horizontally). But two-dimensional cross-classifications result from two different principles of division, where serial arrangement is inadequate. When subdividing, or branching, or ramifying subject-matters become more complicated with relations and interrelations, structures of three or more dimensions, or extensions, may be requisite. Such classifications may be represented diagrammatically or in tabular forms. Subordination and coordination are thus relative and complementary. The tree of knowledge is thus trained to the trellis of structural classification. The fourth structural principle is that serial, branched, and crossed classifications may be combined in structures of three or more extensions. We may term this for convenience the principle of *extension*. The fifth principle, defined by the writer as *collocation* of closely related classes for functional efficiency, is dependent on subordination and coördination.

These five principles of structural classification depend on eight general principles fundamental to classification, which will be

named here without further definition. Some of them are defined or implied in treatises on Logic.[2]

(1) The Relativity of Existence: all things (existents) exist in relations.
(2) The Relativity of Knowledge, in the subject-object relation.
(3) The Correlativity of Classes and Class-concepts, and of names, or terms.
(4) The Relativity of Classes to Existential relations and to Knowledge relations.
(5) Systems are synthetic of classes and relations.
(6) The Relativity and Plurality of Classifications.
(7) Natural and Scientific Classifications should not only be logical but should conform to the order of nature as this is conceived or realized in the consensus of scientists and educators.
(8) In such systems the generic classes are more *permanent* than the specific subdivisions, because defined by fewer relations, whereas the subordinate subject-matters are specified by many terms and various relations. Briefly this may be termed the principle of the Permanence of General Classes.

II. THE ORDER AND THE SYSTEM OF THE SCIENCES

Science is distinguished from communal, common, or "common-sense" knowledge and thought in that it is verified. The single terms *organized*, *systematized*, or *methodic*, as proposed by certain writers, do not suffice to define, nor even to characterize, science. For there may be common-sense methods; and there may be organized systems that are not scientific in method and in verification. An adequate definition of science requires at least seven terms: science is *experiential* and *rational*; it *generalizes*; it is *methodic*, *verified*, *communal*, and *organized*. It is organized systematically. It is communal at least in verification. But above all it is *verified*. Its verification depends on rational

[2] The writer's definitions may be found in the book cited above.

and technical methods, on evidence, and on communal acceptance or *consensus*.

The sciences, general and special, develop from subject-matters comprised by certain comprehensive concepts of classes of objects, phenomena, actions, and events. They involve distinct questions or problems. The object-matters are examined and the subject-matters are treated by methods and technic that are nearly distinctive. Special terminologies become requisite. As the scope and purposes become more definite, the subject-matters are organized about distinctive concepts, hypotheses, theories, and "laws," as nuclei of development. The technics and methods may be extended to relevant technologies. The relevant histories are recorded and communicated or published. Moreover the intellectual results are extended to the relevant philosophic interpretations and speculations.

Subject-matters, or studies, may be classified, but only in so far as they form definite classes. Otherwise they are merely divided, grouped, or arranged. *Groups* are distinguished from classes as being distinct rather than definite, composite but not comprehensive, and usually concrete and localized. Groups are often miscalled classes, and classes are often miscalled groups, or worse called "classifications."

In any classification of sciences and philosophical "disciplines" some of the subdivisions or sub-sciences may be definite subclasses and some may *not*, being single or individual, whereas sub-classes, like classes, imply plural things. Thus Chemistry, one of the Physical Sciences, is a distinct individual science, and Physical Chemistry is a branch of the subject-matter of chemistry, not a sub-class of branches of chemistry, nor of physical chemistry. Its subdivisions, e.g. Electrochemistry, in turn have distinct subject-matters, names, and relations. Likewise Economics is a unitary subject-matter, however subdivided, not a class, not unless the term should be used for a plurality of theories or systems of economics. The same is true of Ethics and Aesthetics, and even of Geometry. Two or three distinct subdivisions that do not form a definite class may thus be groupings and arrangements of groups, but not classes in the proper sense.

This is a reason for preferring to speak of such a structural organization as a *system* rather than a classification.

The historic problem usually termed "the classification of the sciences" is thus misnamed in so far as it has extended beyond defining and classifying the several classes of sciences and related studies, e.g. Formal, Physical, Social, Normative, etc. The term *partition*, used by Bacon and by Wundt (*Einteilung*), implies division in the logical sense. Erdmann's term, *Gliederung*, is intermediate between *Einteilung* and Ostwald's term *System*, which Wundt and Goblot also used. This last term is preferred by the writer as implying interrelations. Münsterberg's term, *synthesis*, for his program of the Congress of Arts and Science at St. Louis in 1904 likewise implied "unification" but not so clearly organization and interrelation, for there may be unification without planned relation of parts to parts and parts to the whole. The olden term, *Encyclopædia*, implies the geometrical unity of the circle but without the organization of the system. Pedagogically it may lack correlation of subject-matters.

It is division and partition that have impaired the structural and functional value of all the philosophical systems from Aristotle's tripartite philosophy to Wundt's dichotomous failure and Münsterberg's seven-branched metaphysical candelabrum. Such logical and metaphysical bases entail much distortion and confusion of valid relations and they are impractical for the functional organization of systems of science, education, and bibliography.

Any system of science should be consistent with the five principles of structural classification defined above. About the most general concepts and main classes develop the *Fundamental* sciences and their derivative sub-sciences, technologies, and applied sciences, also the relevant histories and philosophical extensions. These may be arranged serially and also tabularly, consistently with the principles of subordination and coördination and cross-classification. As the several sciences become more definite in scope, each becomes more or less special in relation to others, and accordingly they may be arranged in a scalar series of *gradation in speciality*, which is our sixth principle, depending especially on

subordination, that is, the terms logically are successively subordinate, while serially they may be regarded as coördinate. In this order Physics is the generalized science of the properties and actions of material bodies, or particles, and of aggregations of these, abstracted from the particulars, accidents, and specifications. Chemistry, a general science too, deals especially with specific properties and actions; it is distinguished by the purpose of specifying, analyzing, and combining specific substances and actions. Biology is the general science comprehensive of the studies of *life*, of vital organic structures and functions. Psychology comprises the more special studies of mental subject-matter. Sociology is the generalized study of the still more special subject-matter of communal organization, relations, and conduct. Social and political history treats of much the same subject-matter in sequential relations and this more specifically and particularly. Economics is the still more special study of human wants and productions. In the very special study of the arts we proceed from those productive of useful goods and utilities to those of pleasurable, reacreative, æsthetic, and intellectual values. Ethics is the generalized study of morals and other special goods in human life and their social and spiritual values, with regard to religious conceptions and relations. All these studies are more or less generalized in their concepts, statements, and theories, but also more and more specialized in scope, subject-matter, interest, purpose, method, and description.

From the fundamental sciences are derived groups, or series, of sciences and sub-sciences. The groups are named after the leading science: thus, the Geological, the Anthropological, and the Medical Sciences. These are *not* definite classes of like sciences. The Mathematical Sciences *are* a class of sciences, or sub-sciences; so also are the Physical, and the Natural Sciences. But, as regards the Biological Sciences, Psychological Sciences, and Social Sciences, the distinctions are unprecise; these groups may be regarded as classes, characterized by subject-matters and problems biological, mental, and social.

Derivative also from the fundamental sciences are: (1) sub-sciences, e.g. Mechanics, and Genetics, (2) applied sciences, e.g.

Hydraulics, and Eugenics, (3) technologies, e.g. Metallurgy, and Pyrotechnics. *Composite* of subject-matters derived from two or more sciences, sub-sciences, or technologies are certain major studies, e.g. Astronomy, and Anthropology, and minor, Chemical Engineering, and Photography. Such composites do not fit simply into either serial or cross-classifications.

Subject-matters may be viewed or treated differently; they may be common to science, technology, history, and philosophy. Not the less they may maintain their individualities, their characters as defined, as developed about nuclear concepts and distinct problems. The series of fundamental sciences may accordingly be crossed by a structure of three or more distinct correlates: (1) sequential aspect, or historical study, (2) application to normative, economic, technical, practical, or artistic purpose, (3) philosophical or critical study.

Interests moreover may ramify to other fields throughout the entire domain of organized knowledge. This extension of interests produces a problem in functional organization for research, for education, and for bibliography. It is for *maximal efficiency* in this that structural classifications should be consistent with the six principles that we have stated. And this *per se* is our seventh principle.

In the table on pages 96–97 the main classes and the Fundamental sciences are shown in the first column; the sub-sciences, composites, and other derived studies are indicated by the terms in the second column, though of course the special histories and many other studies would ramify from these; in the third column are named some of the chief applications, technologies, or arts, and in the fourth column are certain extensions into the fields of philosophy. This cross-classification proceeds from the most general science to the more and more special, that is, as graded by speciality.

This order of the fundamental sciences and major derivative studies is consistent with the order of the sections of The American Association for the Advancement of Science, as designated by the letters A to L, the most important faults in the latter series being that Biology should have place as a fundamental science,

and that Zoölogy should precede Anthropology but not Botany. Our order is consistent also with that of The British Association for the Advancement of Science and with that of The International Catalogue of Scientific Literature, the major errors of the last being that Mineralogy is separated from Chemistry and Bacteriology has been added after certain anthropological sciences, whereas this sub-science is of broader extension. But the differences hardly impair the value of the general agreement of the four orders. They might easily be amended so as to agree completely, with resulting enhancement of values for organizing congresses, programs, subject-matters, bibliographies, and collections in libraries.

The historic roots of this order reach down to the Greeks. Among the Stoic philosophers arose several triadic divisions of philosophy into Logic (to guide the reason), Physics (to explain the world), and Ethics (to rule the social and moral life). This triad was sometimes combined with the prior tripartite division of Aristotle: Theoretical, Practical, and Productive Philosophy. Under the Theoretical were combined Physics, Metaphysics, and Mathematics, and under the Practical were included Ethics, Politics, Economics, and sometimes Rhetoric. Productive Philosophy comprised Poetics and the Arts. Natural History and Psychology were intermediate between Physics and Ethics. With Logic as the fundamental discipline, these triadic divisions resulted in a series of eleven or twelve major studies. With these were afterwards combined the mediæval *trivium et quadrivium*, and later the three faculties of the universities, Theology, Medicine, and Jurisprudence, or Law.

With the comprehensive development of sciences, technologies, histories, and philosophies in the modern age, several other studies arose out of the problems, intellectual and practical: Epistemology, Metrology, Chemistry, Biology, Physiology, Anthropology, Sociology, and Philology. Comte's *hierarchy*, or series of dominant sciences, exalted only six to this first rank. Spencer and Bain and their successors added but two or three: Logic and Psychology, the sub-science Mechanics, and the *composite* Geology. Religion and Philology had been brought forward previously in somewhat divergent systems.

TABULAR SYNOPSIS

Fundamental Sciences, *and main classes of; the histories implied*	Derivative Sciences, *sub-sciences, and composite studies*	Applied Sciences, *Technologies, and studies of Arts*	Philosopy, *special branches; disciplines*
Science in general.	Science of Order, Organization.		Principles. Ontology. Epistemology. Philos. of Science.
A. Abstract sciences (formal). 1. Logic. 2. Mathematics.	Methods, Science of. Statistics. Metrology.		Methodology.
B. Natural sciences. I. Physical sciences. 3. Physics.	Mechanics. Matter and Radiation. Special physics: Heat and Thermodynamics. Light and Optics. Electricity. Rheology. Hydromechanics. Aerodynamics, Pneumatics, and Aviation. Sound and Acoustics.	Technology. Engineering. Physical technol. Electrical technol. Hydraulics.	Philos. of Nature.
4. Chemistry.	Physical chemistry. Crystallography.	Applied chemistry.	
Bb. Natural history.	Mineralogy. Analytical chemistry. Special chemistry.	Petrography. Chemical technol., Engineering, and Industries. Metallurgy.	
	Astronomy, Astrophysics. Geology, Geophysics, etc. Geograhy, Physiography. Meteorology.	Practical Astron. Economic Geol. Economic Geog.	Cosmology.

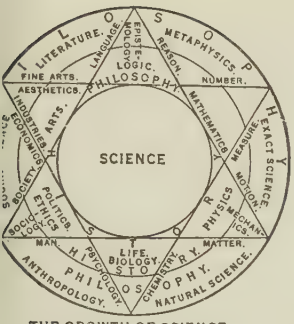
II. Biological sciences.	Biophysics and Biochemistry.	
5. Biology.	Physiology, General.	
	Morphology.	
	Genetics.	
	Ontogeny.	
	Phylogeny.	
	Botany.	
	Bacteriology.	
	Zoölogy.	
C. Psychological sciences.	Animal psychology,	
6. Psychology.	Comparative psychology.	
	Individual and Abnormal.	
III. Anthropological Sciences.	Anthropology.	
	Physical anthrop.	
	Somatology, Anatomy, and Physiology.	
Cb. History of Humanity.	Archæology.	
	Racial anthropology.	
	Anthropological Psychol.	
IV. Social sciences.	Ethnic anthropology.	
7. Sociology.	Psychologic Sociology.	
	Ethnology.	
	Folk-lore.	
	Religion.	
	Morals	
	Political science.	
	Jurisprudence.	
	Economics.	
D. Arts, Knowledge of.		
8. Philology.	Linguistics.	
	Grammar.	

	Philos. of Life.
Eugenics.	
Economic Botany.	
Applied, Medical, etc.	
Economic Zoölogy.	
Psychiatry.	
	Philos. of Humanity.
Medical sciences.	
Hygiene, Recreation, Physical culture.	
Education.	
Euthenics.	
Philanthropy.	Social philosophy.
	Theology.
	Ethics
Politics.	Political philos.
Law.	Philos. of Law.
Economy.	
Useful, Industrial arts.	
Fine, Expressive arts.	Aesthetics.
Recreative arts.	
Literature.	
Rhetoric.	
Poetics.	
Dramatics.	
Theatricals.	
Oratory.	

All these studies may be reduced to twelve fundamental sciences and branches of philosophy. The development may be represented graphically by superposed triangles inscribed in concentric circles. A core of general science is encircled by the fundamental sciences and their major sub-sciences and composite studies in order of gradation by speciality. About this encyclopædia extend the relevant historical studies and beyond these the relevant branches of philosophy. Such geometrical analogies should of course not be mistaken for realities, but they serve to illustrate the historical relations of our poly-dimensional, overlapping, and ramifying, yet unitary structural organization of knowledge and thought. (See page 99.)

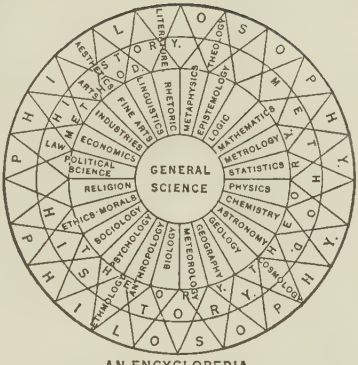
III. BIBLIOGRAPHIC CLASSIFICATION SHOULD BE CONSISTENT WITH THIS ORDER

If the principles stated above are valid and applicable, and if the order of the fundamental sciences and derivative studies, outlined in the foregoing table and more adequately treated elsewhere by the writer, is logically and historically authentic, this structural organization, more fully developed and adapted to practical requirements, should be efficient and valuable for the functional organizations of science and education, research and bibliography. Associations organized in scientific interests have already embodied this order, but not all of them consistently and none in complete consistency. Surveys in books have usually in recent years conformed to it broadly. In educational institutions and curricula there has more recently arisen an important movement toward correlation of studies consistently with this order. Subject-matters are to be grouped and studied in relation to more general sciences. Specialization, focused on minute analysis of facts, has lacked comprehension of relations, without which the handling of large and complicated problems proves inadequate. But now intellectual synthesis and organization of subject-matters and studies are purposed not only in liberal education for grounding of intelligence but are regarded as prerequisite to scientific and technical mastery of the intricate relations of nature and human nature.



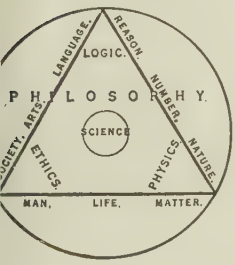
THE GROWTH OF SCIENCE
IN RELATION TO
HISTORY AND
PHILOSOPHY.

THE FUNDAMENTAL SCIENCES
AND RELATED BRANCHES OF
HISTORY AND PHILOSOPHY

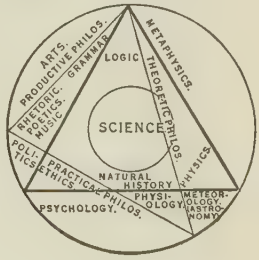


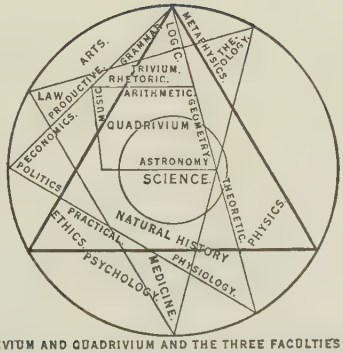
AN ENCYCLOPEDIA
OF TWENTY-FOUR SCIENCES AND BRANCHES OF
HISTORY AND PHILOSOPHY

THE STOIC TRIAD



ARISTOTLE'S TRIAD
COMBINED WITH THAT OF THE STOICS.





THE TRIVIUM AND QUADRIVIUM AND THE THREE FACULTIES OF THE
MEDIEVAL UNIVERSITIES, COMBINED WITH THE STOIC AND ARISTOTELIAN TRIADS.

(*Printed by permission of Henry Holt and Company*)

Bibliographic systems should be consistent with the systems of science and education which they serve. But books and libraries are less plastic than courses of study and curricula and less individualized than most scientific research. They must serve many readers and studies. These, however, are reducible to statistical averages, types, and norms, which are less variable than individuals, groups, and varieties; and they are more permanent. Bibliographic systems too should be more permanent, because they pertain not only to present resources but to past and to future. Herein are reasons why classifications that are purposed for general and typical uses should be consistent with systems that are normal to the stable consensus of scientific and educational minds. In this they should be more consistent than educational institutions need be and scientific associations need purpose to be. In short, "maximal efficiency" in a bibliographic system is even more requisite than the logical coherence on which it depends, though for intellectual values this is no less important.

But bibliographic systems have been developed by bibliographers and librarians, who have recognized neither these needs nor these values and who have disregarded, or ignored, the principles of logical classification and the systems of science and education. British, American, French, Belgian, and Italian bibliographic classifications have been based upon the arbitrary, antiquated systems of Bacon and Brunet and adaptations of these in the earlier decades of the nineteenth century. Certain German systems have, however, been comparatively independent, though not much better, for reasons not very different.

When at the beginning of this century the international organization of intellectual interests became a great movement, the International Institute for Bibliography projected a comprehensive classified *Répertoire* but unfortunately espoused the leading American system, the *Decimal Classification* of Melvil Dewey. This ingenious instrument was devised by an undergraduate at Amherst College in 1873 by a "stroke of genius"—simply applying a notation of decimal numbers so as to provide for sufficient expansion and then alphabetically indexing the inherent and ensuing disorder. This maze separates Science (Class 5) from Phi-

losophy (Class 1), Social Sciences (Class 3) from History (Class 9), and Literature (Class 8) from Philology (Class 4). As an example of misrelated subordinate subjects space permits only one among hundreds: in the Social sciences Political Science (320) is separated from Administration (350) and from Law (340) by the whole of Economics (330), while Commerce and Communication are remote in 380. Similar disorder appears throughout the thousand subdivisions, which remain almost unchanged, though "expanded" with much detail. Three of the fundamental sciences, Biology, Psychology, and Sociology, have not been developed as distinct but have been subordinated or dismembered among other subjects.[2]

The defenders of this entrenched system reply that logical order is academic, disputations, impractical, and unimportant, provided there be an alphabetic index to locate the subjects by their numbers. Granted, an index locates terms in schedules and books on shelves, but it does not *classify* either subject-matters or books. The notion and the argument that it does the writer has termed "the subject-index illusion." This ignores the basic principle of subordination of the more special subject-matters to the more general, the principle of gradation in speciality, and that of collocation of related subjects for "maximal efficiency." An alphabetic index is a requisite complement to any complex systematic classification, but it should not be substituted for the classification. It would be more direct to substitute an alphabetic classification for the index of terms. But alphabetic order is dispersive, the very antithesis of classification. Moreover it depends on the complications of terminology, which confuse economies and impair utilities. In the English language terminology is inconsistent beyond compare. In international systems the confusion becomes ten times confounded. Yet the minds of bibliographers and librarians, particularly in America and in Great Britain, have been alphabetically predisposed. The crucial question is whether the knowledge in books, in bibliographies, in libraries, in museums, and in other educational institutions

[2] These criticisms are amplified and justified in the writer's second book, *The Organization of Knowledge in Libraries*, published in 1933.

should be organized systematically, logically, and scientifically, or alphabetically under "the subject-index illusion."

The International Institute's adoption of the Decimal Classification some 35 years ago, for the specious reason that no better system was then available and because the decimal notation seemed very international, was the most discreditable event in "the historical situation" in which this problem has been immersed. There was nothing else international about the Dewey classification. It was intensely American, and it has remained so. Yet on this basis the International Institute has elaborated an immense bibliographic undertaking, which would arrogate to be "standard" and "universal." The efforts in the elaboration and the propaganda might better have been expended on developing and publishing an international coöperative bibliographic system consistent with principles and the systems of science and education. That system is *not*.

But there is a vigorous awakening in Europe that would proceed to this purpose. The International Institute has voted to appoint a commission to study the problem considerately in its various aspects. If new structures be requisite, if new foundations are to be laid, the details that have been compiled for the systems now serving unsatisfactorily would then be available for the reconstruction.

If science be regarded as organized knowledge and philosophy as organized thought, grounded on scientific knowledge, though extending beyond it to speculative realms, the structural organization for knowledge and thought which has been outlined in this article may be regarded as the logical fabric of a *microcosm*, mental, intellectual, and scientific, which is consistent with and correlative to the intrinsic physical organization of the *cosmos*, the *macrocosm*. Whether this objective cosmos be *real* is not here the question, nor even whether a conceptual correlation, or microcosm, is intellectually valid. But, *assuming* the reality of the cosmos and the valid conceptual correlativity of the microcosmos, the question is whether our intellectual resources and studies should not be organized coherently with it. If so, it behooves the associations and institutes that envisage these in-

tellectual problems to consider earnestly the proposals to reorganize the bibliographic systems with regard to scientific and educational interests. If not, what is the alternative? Can intellectual purposes be served more satisfactorily in "freedom" by unorganized, inconsistent, confused contrivances with undefined terms and tangled terminologies? Are the revelers in kaleidoscopic views drifting onward to be submerged in unending futile disputation? Come; let us abjure this unreasoned pessimistic negation!

The College of the City of New York,
Convent Avenue and 139th St.,
New York, N. Y.

DISCUSSION (CORRESPONDENCE)

In this section we shall publish brief communications commenting on published papers or bringing up special points of interest which can be discussed by correspondence.

CONCERNING THE METHOD OF NUMBER-PAIRS*

Dear Sir:

Prof. E. T. Bell has given in his greatly stimulating essay "Finite or Infinite" (Vol. 1 no. 1 of this Journal), an introduction to the current discussions concerning the foundations of mathematics, which bears a very special character. Perhaps after all the seriousness and even the pedantry, in which these differences of view are frequently fought out with the fanataicism of religious conflicts, a superior, humorous, and at times ironical note as that of Dr. Bell might really contribute to diminish the obstinant partisanship and to further the will to mutual understanding. Dr. Bell's sympathies belong unmistakably to a certain school, which can be denoted by the diagonal of a parallelogram of force, whose sides are formed by different intuitional tendencies. Although I am not able to follow him in his fundamental orientation, which he has explained in a most profound manner in another place, I would nevertheless like to forego at this point entering into a controversy concerning the paramount problems of principle.[1]

The aim of the following lines is merely to take issue with a single point of Dr. Bell's article, namely the method of *enlarging the concept of*

* Translated by Haym Jaffe.

[1] I hope to be able to express myself soon about them in a new edition of my "Einleitung in die Mengenlehre" (the last being the 3rd Edition, Berlin, 1928). For the present I wish to refer to my paper in *Erkenntnis*, Vol. I, furthermore to the papers by Carnap, Heyting, and von Neumann in Vol. II of the same journal, and to my brief note, "On Modern Problems In The Foundations Of Mathematics" (*Scripta Mathematica*, Vol. I), as well as to M. Black's "*The Nature of Mathematics*" (London, 1933).

number through the construction of number-pairs, which is brought up there especially for the introduction of rational numbers.[2]

In the cited article it is stated in relation to the method of number-pairs (p. 33): "The spirit of it goes back to Gauss (1777–1854),"[3] and later it is explained, that the well-known introduction of complex-numbers is thought of (p. 38). In reference to the questionable method of Gauss the opinion of a "professional mathematician" is quoted as follows, "I am disgusted." I wish to show in the following lines that a similar opinion had already been expressed by the *contemporaries of Gauss* in reference to this method, but that it *does not at all concern itself with the method of number-pairs*. This method did not arise on the continent, but in England, and is even today generally accepted; it can indeed be defended even against the main objection of Dr. Bell.

Gauss, as is well known, has established complex numbers in 1831 in the announcement of his second treatise about the Biquadratic Residues; the treatise itself, in which complex numbers are applied for the purpose of the Theory of numbers, is printed in Book II of Gauss's Works, p. 93–148, the announcement in the same volume, p. 169–178.[4] To be sure, several decades before, C. Wessel, of Denmark, presented in a treatise before the Copenhagen Academy in 1797, and which appeared in the Transactions of the Academy in 1799, an introduction to the complex numbers identical and of equal value with Gauss's proof. Wessel's work, however, remained completely unnoticed, until it was accidentally discovered in 1895 and had been published again, and likewise a work by J. R. Argand, very much to the point, which appeared in 1806, met with little attention, so that it was Gauss who with his aforementioned treatise gained full recognition within the field of mathematics for Complex Numbers on an equal plane with real numbers, both historically and in the minds of mathematicians.

[2] On this occasion permit me another remark of detail: on p. 42 Dr. Bell renders the translation of the famous Cantorian definition of an aggregate (which is soon after quoted verbally). At the same time the words, "Our intuition or our imagination" have according to their meaning been introduced in a wrong place. These words refer in Cantor to the *origin* of objects (consequently an entirely subordinate point), and not the central act of comprehension of the whole.

[3] Incidentally Gauss did not die until Feb. 23, 1855.

[4] Compare with reference to the content and the criticism of the Announcement, my essay "*Materialien für eine wissenschaftliche Biographie von Gauss*" which appeared in Vol. VIII, "Zahlbegriff und Algebra bei Gauss" (printed in the *supplement* to the *Nachrichten der Gesellschaft der Wissenschaften zu Goettingen*, Math. phys. Klasse, Jahrgang 1920.

The method of this justification is however that which Georg Cantor calls a "transient" proof in contrast to an "immanent" one; the admissibility of complex numbers is not demonstrated within arithmetic itself, but through the allusion to the possibility of rendering *geometrically* perceptible the complex numbers together with the purely real ones, namely as points or vectors (Gauss speaks of the 'transitions' between the points) in the now so-called Gaussian Number plane. At the same time the imaginary unit i appears as a middle proportional between the real units $+1$ and -1. The fact of equalization between the thus obtained spatial representation of the purely real and the complex numbers is the argument on which Gauss's "plaidoyer" takes as a basis to plead in favor of complex numbers (which were then already used in Mathematics for some time, but no more than tolerated as being not quite legitimate). That Gauss was well aware of how unsatisfactory this proof derived from arithmetic is in itself is shown by his letter to Drobisch, August 14, 1834,[5] in which he writes: ". . . But the presentation of imaginary magnitudes in the relations of points in plano is not so much their own reality, which must be understood on a higher and more universal plane, as rather the purest or perhaps for us human beings the one singularly and completely pure example of its application." Gauss therefore refrains from an immanent proof of complex numbers, remaining in the realm of numbers itself, not that he would not admit the merit of such a proof, but because he sees no way to it. In connection with this point one must surely speak of a defect of the "Princeps Mathematicorum," no matter of how much consequence such a proof has become for all of Analysis.

Gauss's younger contemporary, Johann Bolyai, the independent co-discoverer of non-Euclidean Geometry, has indeed raised a series of weighty objections against Gauss's proof of complex numbers, in a prize-essay presented in Hungary as early as 1837, and not published until 1899. Among them let only two be mentioned: A reproof for the use of space as evidence for arithmetic, and an objection against the arbitrarily selected quadratic lattice-arrangement of the plane, on which the property of the middle proportionality of i is based, which can however be just as well replaced by a rhombic one, for example. Bolyai himself attempts a purely arithmetic proof which nevertheless leaves much to be desired in simplicity and clarity.

He could not know, that in the same year (1837), in which he pre-

[5] Gauss' Werke, Bd. X., p. 106.

sented his prize-essay, there appeared a paper by W. R. Hamilton, which had already been read in the preceding year before the Irish Academy,[6] which offered a *straightforward completely pure arithmetic proof of complex numbers*. It is just the proof according to the method of *number-pairs*, in somewhat the same form as it appears for the proof of rational numbers in Dr. Bell's essay. The problem, *why* this proof leads to the goal with just this and with no other definitions of the utilized relations, and how one can logically attain the necessary construction of the definitions under question lay outside of the prevailing scope and was not treated until Weierstrass and his followers did. The logical inviolability of the Hamiltonian proof is left intact.

Finally a brief remark concerning Dr. Bell's doubts (p. 36), as to whether the *ordered* pair (a, b) is defined beyond objection, and therefore can be particularly distinguished from the pair (b, a). The possibility of such a distinction is an essential stipulation for the fact that one can apply the method of pair-construction in arithmetic. I do not wish to discuss the psychological or even physiological arguments. Regarding them one might perhaps emphasize that through human nature we receive sense-impressions only in temporal *succession*, and that we are able to fix symbols only in spatial succession, and that consequently simultaneity represents a logical abstraction from an originally given series. According to that the *unordered* pair would be secondary in relation to the primary concept of the *ordered* pair. But if we wish to limit ourselves to logical mathematical arguments, then the simple reference shall perhaps suffice that when objects a and b are given, the following two complexes can be defined from them as ordered pairs: ((a), (a, b)) and ((b), (a, b)). These complexes are easy to distinguish logically; if one perhaps defines the first as the ordered pair from, first, a and, secondly, b, (or what amounts to the same, as the asymmetrical relation $a < b$), then the second complex becomes of itself the reversed ordered pair. A generalization of this procedure is incidentally common in the axiomatic Theory of Aggregates, when the problem is to deduce the concept of an *ordered aggregate* from the concept

[6] Transactions of the Irish Academy—vol. 17, p. 293 ff.... The treatise at first remained unnoticed, and became known to a larger circle first through Hamilton's Preface to his *Lectures on Quaternions* (Dublin, 1853). Incidentally in an investigation not published by him and probably originating as early as 1819 (published in Gauss's Werke, Bd. VIII, p. 357 ff), Gauss worked with quaternions even *before* Hamilton, and introduced them as "combinations" of four real numbers without however as much as throwing a side glance on arithmetic, in his investigation devoted to "Mutations of Space."

of an *aggregate as such*, without the introduction of a new logical fundamental concept (such as that of order).

ADOLF FRAENKEL.

Hebrew University, Jerusalem,
Einstein Institute of Mathematics.

Dear Sir:

Through the kindness of Professor Fraenkel, I was enabled to see his note before publication. There are one or two remarks which may clarify some of the issues suggested in my article.

I must confess to a difficulty in understanding the various interpretations that have been given to Cantor's definition of "Menge." Perhaps one accepted English translation (E. W. Hobson, *The Theory of Functions of a Real Variable*, Art. 111) will clear up the matter: "A collection of definite distinct objects which is regarded as a single whole is called an aggregate." It will be noticed that "collection," as an English word, could have two distinct meanings—the uncompleted act of collecting, or the result of a completed act of collection. Likewise the word "regarded" is ambiguous. However, I gave Cantor's exact words, although I have not yet any clear idea of what the German words "Anschauung" and "Denkens" mean; an extreme behaviorist might include both words in the category of meaningless noises. Similarly for "regarded," which is metaphorical. The metaphor is unresolved. The apparent fact that there seems to be still some doubt among experts as to the precise meaning of "class," or of "Menge," indicates that something remains to be done in the way of definition. One possibility is to accept "Menge" intuitively; another is to add further definitions of the terms used in the official definition. I believe the net result will be the same: those to whom the original definition is free of mysticism will be confirmed in their insight, while those who lack insight originally will not be enlightened.

It seems to me that this extremely elementary matter of what a "Menge" is, is the parting of the ways, where those who are capable of belief in what is not humanly constructible go to the right, while others turn to the left. The leftists may agree that the words sound like sense but are reluctant to credit the words with more than sound. Few mathematicians, however, are such extreme leftists as this, and even the most skeptical do not let their lack of beliefs interfere with their technical mathematics—which, according to their philosophical nihilism, may be totally devoid of meaning or consistency.

Even after Professor Fraenkel's lucid remarks on the possibility of distinguishing the *ordered* pairs (a, b), (a, b), I still believe an appeal to extra-mathematical elements (psychological, say) is made in affirming the possibility. Although Professor Fraenkel disavows any intention of going into the psychological or physiological aspects of the question, it seems to me that his own description contains at least some such elements. Further, if we as mathematicians are later to found precise ideas of simultaneity and spatial sequence on the number system, it would seem that we are likely to run into circularities if we appeal to these very ideas in constructing the number system. Would it not be simpler, as some of the formalist-logisticists do, to postulate that a "sign," like (a, b), for instance, has a definite, recognizable "individuality," and that "distinct" signs, like (a, b) and (b, a) are "recognizable" as "distinct"—if they "are" distinct. Although it might be simpler, it would none the less be a highly sophisticated appeal to elements which are not usually included explicitly in the postulate systems from which we construct or deduce the real numbers. In any case, I believe it should be stated what our starting point is to be—marks on paper and verbal rules (not always set out as part of the official formalistic program) for manipulating them, or an intensional attempt to put some meaning into or behind our marks. I do not recall any attempt to do this seemingly simple thing which has withstood destructive criticism to the extent that what is left of the attempt is commonly accepted by a majority of specialists in the foundations of mathematics.

In this connection, Professor Fraenkel's allusion to Cantor's distinction between an "immanent" and a "transient" foundation, brings out sharply one of the points on which leftists and rightists appear to differ beyond hope of reconciliation. The distinction in question is reminiscent of mediaeval doctrines of the theological infinite, which may be appropriate in the philosophy of one who, like Cantor, was sympathetic toward such doctrines, but which, nevertheless, seem curiously vestigial in a mechanized civilization. A few years ago such doctrines reached one of their recurrent high water marks in English-speaking countries with the enthusiastic reception accorded Evelyn Underhill's *Mystic Way*. I believe it was the war which put the quietus on the widespread revival of mysticism inspired by Miss Underhill's work, by demonstrating in a pragmatic fashion that the scholastic philosophy does not suit the brutally direct modern mind. It may be that a little more of such scientific psychology as we have and a little less mediaeval theology in the beginnings of mathematics may be just what mathematics needs in order to

bring it down from heaven to earth and enable it to reach a humanly acceptable agreement with itself.

In ascribing the *spirit* of the method of ordered pairs to Gauss, I thought I was on fairly safe ground, because it seemed to me that the passages from Gauss quoted presently are as strong a justification for the claim that he had the *spirit* of the method as is some of the evidence put forward by the editors of Gauss' works for other anticipations by the "Princeps mathematicorum."[1]

It seems to me that the man who first clearly saw the two-dimensionality (not necessarily a geometrical intuition) of the complex number $a + bi$ recognized that he was handling an ordered pair of real numbers a, b, the ordering being implicit in the definition of equality for such complex numbers, namely that $a + bi = c + di$ (a, b, c, d, real) when and only when $a = c$ and $b = d$, had the root of the matter. The imaginary unit directs that the reals be correlated in a definite, ordered way, and if we write (a, b) for $a + bi$, and impose then the definition of equality for these particular ordered pairs, $(a, b) = (c, d)$ when and only when $a = c$ and $b = d$, we add only a new notation to what we were doing already, namely manipulating ordered pairs. The first quotation contains no allusion to the definition of equality which (it seems to me) introduces the notion of ordered pairs, nor can I find that Gauss anywhere set down the postulates for equality of complex numbers or the hypercomplex numbers which he discussed. Nevertheless, the germ of the idea does seem to me to be in the first extract, and without the full-blown idea itself I do not see how we are to interpret the equality stated by Gauss in the second extract (on which, in part, his claim to have anticipated Hamilton in the invention of quaternions is based). As the remark "I am more interested in notions than in notations" is sometimes fathered on Gauss, I thought the "notion" in the first quotation, which is independent of the geometrical interpretation of complex numbers, and which blossoms out into the "notation" for hypercomplex numbers in the second, justified ascribing the idea to Gauss. I may say that I was thoroughly familiar with the historical sources cited by Professor Fraenkel, including his own extremely interesting paper on materials for a scientific biography of Gauss, when I wrote my paper.

"Sind aber die Gegenstände von solcher Art, dass sie nicht in Ein, wenn gleich unbegrenzte, Reihe geordnet werden können, sondern sich

[1] See, for instance, as an illuminating essay in reconstructive historical criticism, Gauss' Werke, Bd. X2, pp. 93–105, especially p. 104, and the references there given.

nur in Reihen von Reihen ordnen lassen, oder was dasselbe ist, bilden sie ein Mannigfaltigkeit von zwei dimensionen; verhält es sich dann mit den Relationen einer Reihe zu einer andern oder den Uebergängen aus einer in die andere auf eine ähnliche Weise wie vorhin mit den Uebergängen von einem Gliede einer Reihe zu einem andern Gliede derselben Reihe, so bedarf es offenbar zur Abmessung des Ueberganges von einem Gliede des Systems zu einum andern ausser den vorigen Einheiten $+1$ und -1 noch zweier andern unter sich auch entegegengesetzen $+i$ und $-i$. Offenbar muss aber dabei noch postulirt werden, dass die Einheit i allemal den Uebergang von einem gegebenen Gliede einer Reihe zu einem *bestimmten* Gliede der unmittlebar angrenzenden Reihe bezeichne. Auf dies Weise wird also das System auf ein doppelte Art in Reihen von Reihen geordnet werden können."—Gauss, *Werke*, vol. II, p. 176.

The "notion" appears also in Gauss' *Werke*, vol. VIII pp. 359–60, where the "combination" a, b, c, d is denoted by (a, b, c, d) and "we write"

$$(a, b, c, d)\ (\alpha, \beta, \gamma, \delta) = (\mathrm{A}, \mathrm{B}, \mathrm{C}, \mathrm{D}),$$

etc. According to the Editor (Stäckel), internal evidence assigns this fragment to about the year 1819, which is before the date of Hamilton's first published discussion of ordered pairs. That Gauss, in this equality, had broken away from geometrical intuition, seems evident, as he did not discuss 4-dimensional "space."

E. T. Bell.

California Institute of Technology.

A NOTE ON THE VALIDITY OF ARISTOTELIAN LOGIC

Dear Sir:

In a recent issue of this periodical,[1] there appeared an article by Mr. L. Kattsoff entitled "Concerning the Validity of Aristotelian Logic." By use of the formula for the A proposition, $\mathrm{A(ab)} = (a < b)\ [(b < a) + (a < b')'\ (b' < a)']$[2] and corresponding formulae for the E, I and O types, Mr. Kattsoff gives the method of Dr. H. B. Smith of establishing the complete generality of Aristotelian logic, its consistency, and the validity of all the classical forms of inference. That Dr. Smith's

[1] *Philosophy of Science*, Vol. 1, No. 2, pp. 149–162, April, 1934.

[2] *Loc. cit.* p. 157. "$<$" and "$'$" have their usual meaning in the algebra of classes. "$+$" indicates a disjunction of propositions.

system is formally consistent there can be no doubt, that it permits all the forms of inference sanctioned by the classical treatment of the syllogism and immediate inference, is clear, but that it therefore constitutes a validation of Aristotelian logic depends on the highly dubious assumption that Aristotelian logic consists merely of these doctrines of syllogism, immediate inference and the square of opposition. In particular, it must be assumed that the laws of contradiction and of excluded middle are not a part of Aristotelian logic, since, as a slight examination will show, Dr. Smith's system assumes both of them to be false.

The law of contradiction may be stated in some such terms as the following: No proposition of the form "S is both P and non-P" can be true.[3] From this we might expect to infer for example, that "No entity is both red and non-red" and according to Dr. Smith's system this is a true proposition.[4] Any further specification, however, results in a false proposition. Thus the statements "No chair is both red and non-red," "No geranium is both red and non-red," "No round-square is both red and non-red" are all false and their contradictories true.[5] This not only involves a breakdown of one of the laws of logic most firmly established by tradition, but also is repugnant to common sense.

Further paradoxes await the enquirer who raises the question "If it is true that some chairs are both red and non-red, which chairs are?" For according to Dr. Smith's analysis, the propositions "This chair is both red and non-red" and "This chair is not both red and non-red" are both false however the chair in question be chosen.[6] This involves a denial of the law of excluded middle as well. Thus we are left with a situation in which no entities are both red and non-red, but some of every species of entity are both red and non-red and with regard to any individual entity, it neither is nor is not red and non-red.

[3] Some writers restrict the law to a relation between two contradictory propositions. *Cf.* Keynes, *Formal Logic*, 4th ed. p. 454. Sigwart, *Logic.* Translation by Dendy, Vol. I, p. 139. Most writers, however, admit some statement closely approximating the above as at least one if not the only formulation of the law. *Cf.* Bain, *Logic*, Vol. I, p. 16. Joseph, *Introduction to Logic*, 2nd ed. p. 16. Eaton, *General Logic*, p. 423. Stebbing, *Modern Introduction to Logic*, p. 469. In any case no writer would challenge the principle as stated above and it is definitely Aristotelian. Cf. Aristotle *Metaphysica* 1006 a-7.

[4] Since the class of entities is the universe class, the class of things which are both red and non-red, the null class; and E(1, 0) is true. See the tables given on p. 160, *Loc. cit.*

[5] According to the formulation given, E(ab) is false when $b = 0$ and $a \neq 1$. *Loc. cit.* p. 160.

[6] Assuming the traditional analysis of the singular proposition as a universal. If the singular be considered a particular, both the above propositions are true.

Because of the above paradoxes, it is difficult to see how Dr. Smith's system can be accepted as the equivalent of Aristotelian logic or how therefore it contributes to the establishment of the validity of that logic.

PAUL HENLE.

Smith College.

Dear Sir:

I am glad to reply to Dr. Henle's sympathetic criticism because, where issues can be so tightly drawn as in logic, there is great hope of coming to an understanding.

Dr. Henle's statement of the "law of contradiction," because of its ambiguity, leaves me somewhat puzzled. He states it thus:

"No proposition of the form 'S is both P and non-P' can be true."

(1) If S is a class with members the law holds,

(2) If S is an empty class the law fails,

(3) If S is a singular term (which seems to be implied by the reference to Aristotle) the meaning of the copula has changed to what is currently represented by ϵ.

In this last case the criticism would be irrelevant. Moreover the pretended cases, No entities are red and not red, No chairs are red and not red, are not instances of the law as stated.

Whether in the passage which Dr. Henle refers to, Aristotle has in mind a "law of contradiction" is, to say the least, problematical. At any rate the usual word for contradiction is not used. I quote the Organon in the Latin (Analyticorum Post. Lib. I, Cap. II, 12) because the word comes to us through the Latin:

"Contradictio autem est oppositio, cuius non est medium secundum ipsam. Pars vero contradictionis illa, quae aliquid de aliquo (enuntiat) est affirmatio; quae autem aliquid ab aliquo (removet) negatio."

The form of the "law" which Dr. Henle would have us accept may be used by writers who tacitly assume that the classes in question are not empty. In the original passage (Met. 1006 a) the reference is to a singular term, a *something* which cannot both be and not be (again a different use of the copula).

If the law of contradiction be stated:

P (is true) and P (is untrue) is impossible,

What is P and non-P is non-existent,

it does not break down on my interpretation of the categorical forms; and similarly for the law of excluded middle:

Everything (in the universe) is either P or non-P.

In introducing singular terms Dr. Henle has made the universe of discourse ambiguous (see Dr. Singer's article, this Journal, Vol. I, No. 3). This chair is red and not red, this chair is not both red and not red, would be expressed in current notation:

$$C \in O \text{ (is true)}, \quad C \in O \text{ (is untrue)},$$

and these we have not asserted to be true together.

If the class chairs has members and is not the universe, then the answer to Dr. Henle's question:

"Which chairs are both red and not red?"

is: "All the non-existent ones." Perhaps if "Some chairs are red and not red" be put in the equivalent form, "Some chairs do not exist," it will appear commonsensible enough.

If Dr. Henle feels that such replies are repugnant to Common Sense, we reply: So is the denial of subalternation repugnant to Common Sense; and if we are to save this for Common Sense we shall have to give up the other. Common Sense and Mother Wit will have to be prepared to make sacrifices in the interest of science when they contradict themselves.

Dr. Henle agrees that my intrepretation of the universal affirmative retains the traditional doctrine of the syllogism, immediate inference and the square of opposition, and we have shown that there is no violation of the laws of contradiction and excluded middle properly stated. We think this suffices and justifies us in terming the generalized system Aristotelian.

Henry Bradford Smith.

University of Pennsylvania.

REVIEWS AND NOTES

THE TROUBLED WATERS OF MATHEMATICS

The Nature of Mathematics. A critical survey. By Max Black. London, Kegan Paul, Trench, Trubner and Co., Ltd.; New York, Harcourt Brace and Co., 1934. Pp. xiv + 219.

Both professional mathematicians who are not specialists in the foundations of mathematics and non-mathematicians broadly interested in the philosophy of science will find in this book an extremely clear and comprehensible discussion of the main problems in current work on the foundations of mathematics. Even experts in one of the three leading schools may find a reading of the author's impartial statements of the aims and achievements of the other schools a salutary caution against embracing the blind bigotries and intolerant dogmatisms which have disfigured the work of some writers in the several factions. The fact that it is possible to produce a temperate work like the present in the midst of hotheaded controversies and profitless logomachies on the nature of mathematics is, let us hope, a sign that the polemical phase is nearly ended, and that *all* workers in the foundations are about to concentrate their attention on an attempt to understand one another instead of squandering their truly amazing inventiveness on the manufacture of ever newer and more offensive epithets to hurl at their rivals' irascible heads.

The three leading schools (a fourth, in Poland, is rapidly forging to the front) are the logistic, associated with Whitehead and Russell, the formalist, inspired by Hilbert, and the intuitionist, almost wholly the creation of Brouwer. To retail the aims and accomplishments of these schools here would be to paraphrase the author's book. As the book is not only clear but also remarkably concise, a paraphrase would consist largely in transcripts of passages, which would spoil the reader's enjoyment of the book itself. However, one or two highlights may be briefly noted.

It has been apparent for some time to students of Whitehead and

Russell's attempt in *Principia Mathematica* to reduce pure mathematics to a branch of logic that the attempt is still only an attempt. The author goes farther and, while giving an unbiased account of criticisms which have been aimed at the logistic program during the past twenty years, appears to make it plain that the strict form of the logistic attack cannot be carried through. Nevertheless it would be a mistake to say that *Principia* was all a mistake; nor does the author follow some critics of logistics in making so sweeping—and unjustifiable—an assertion. The traces of *Principia* are apparent on practically every page of the writings of the formalist school, which indeed derives historically from two principal sources: the early postulational work of Hilbert, and the very work of the Whitehead-Russell school which the formalists seek to supercede. Indeed, it has been remarked that the 1928 book of Hilbert and Ackermann is in many respects little more than a rather naïve *Principia* with hints of the matrix method (invented, it would seem, by Post, whom our author does not list in his bibliography).

To the reviewer the highlight of the discussion of the work of the formalists is Gödel's theorem, published in 1931, which demonstrates the impossibility of proving that mathematics is free of contradictions. Roughly, the formalists, until they begin "metamathematicizing," hold that pure mathematics is the science—or syntax—of formal ("meaningless") structures of symbols. One objective of the school is to prove that manipulations of the symbolism will not produce a contradiction, the latter being some such "formula" as $1 = 0$. Gödel's result would seem to show that this objective is illusory. However, in their 1934 book, which appeared after Black's was printed, Hilbert and Bernays, in briefly alluding to Gödel's destructive work, seem to promise that the difficulty will be overcome in the second volume—which has not come out yet. It will be interesting to all students of the foundations to see what finally will be the outcome.

The aims and the philosophy of the third school, the intuitionist, have probably given professional mathematicians more trouble than all the work of all other schools combined. One expert, a fellow countryman of Brouwer's, has even assured his helpless colleagues that a thorough understanding of Brouwer is impossible to anyone who does not think in Dutch. The intuitionists postulate a "basic intuition" of the possibility of "constructing" an *infinite* sequence of numbers. This is intelligible to most mathematicians, or rather, most mathematicians think it is intelligible to them. What comes out of it is not always by any means so easy to grasp—at least for mathematicians. The

present book should do much to clarify some of Brouwer's more profound ideas to those who struggle to understand him. A pleasingly unorthodox feature of the intuitionist program is its insistence upon the humanly social, rather than the celestially unsociable, nature of mathematics. All this is clearly explained in the book.

There is only one point—a statement of fact—on which the reviewer would venture to disagree seriously with the author. It cannot be treated adequately here, but may be taken up elsewhere (if the reviewer is as right as he thinks he is at the moment; he may be quite wrong). This concerns the account on pp. 176–7 of Kronecker's program for basing all mathematics on the common whole numbers 1, 2, 3, . . . In particular, Kronecker would abolish the negatives, $-1, -2, -3, \ldots$, from rigorous mathematics. If Kronecker's words as reported on p. 177 concerning the "magnitude" defined by $x + 1 = 0$ really mean what they are said to mean, then Kronecker was incredibly stupid. But it is well known that Kronecker was an algebraist and an arithmetician of extraordinarily acute penetration. The blunder of making him say something that is no better than a childish stupidity, as Couturat did, would be possible only to one who, like Couturat, had paid indifferent attention to Kronecker's mathematical background. That background is based, in part, on Kummer's conception of ideal "numbers," where the important thing to note is the quotation marks on "numbers." Ideal "numbers" are not numbers (like 1, 2, 3, . . .); to speak of an ideal "number" having such-and-such "properties," or "existing," is merely a mode of speaking, used for brevity; no "numerical" context is presupposed or implied and the use of "number" is only a convenience (perhaps unfortunate) to express concisely certain behavior of *congruences*. The like applies to Kronecker's "magnitudes," where the quotation marks rescue his assertion from being nonsense.

In addition to clear accounts of other men's work, the author illuminates the discussion with criticism and suggestions of his own. The word "critical" in the subtitle is fully justified. It is to be hoped that the author will find an early opportunity for elaborating his own contributions, particularly those in the Supplementary Note on Logical Analysis. If "structure" can be made clear, we may see something.

E. T. Bell.

Department of Mathematics,
California Institute of Technology,
Pasadena, Calif.

The Search for Truth, by Eric T. Bell. Baltimore: The Williams & Wilkins Co., 1934, pp. x + 279.

This book retraces the intellectual evolution of the human race in its struggle for a science of thinking. It reveals the pinnacles which have been reached during the last four thousand years and exhibits what human beings in successive eras have imagined truth to be. But the end is not yet! For the ancient query of Pilate, *What is Truth?* is still unanswered. New answers are yet to be considered. The science of mathematical proof presents its reply, and so it is necessary to examine this mysterious stuff called mathematics to see whether the latest answer makes any better sense than its predecessors. It turns out that Pilate's question has no answer, and thereby hangs a tale—and perhaps a tragedy. Bertrand Russell turned from mathematics to philosophy for an answer to the question, *Is mathematics true?*—only to discover that Pilate's question is meaningless noise!

The main thesis of the book is that human thought, hog-tied for over two thousand years by Aristotle and Euclid (someone should investigate the logical interdependence of these two schemes of thought), has now reached a point where a further emancipation is desirable. The three curses which the ancients put upon subsequent thinkers are (*a*) the belief in *superhuman truth*, (*b*) the love of *absolutes* (e.g., the Aristotelian "laws of thought") and (*c*) the fallacy of *extrapolation*. These, along with the curse of *verbalization* (philosophy), provide the basis for endless and futile systems of thought. Bell contrasts the *talkers* with the *doers*, the latter, apparently, being those who employ the operational theory of meaning (Bridgman). And just as Lobatchewsky (in 1826) emancipated mathematicians from the idea of absolute truth in geometry, so Lucasiewicz, and later Lucasiewicz and Tarski (in 1930), emancipated thought from slavery to Aristotelian logic. Bell states that to the best of his knowledge the Dutch mathematician L. E. J. Brouwer was the first to challenge the Aristotelian law of excluded middle, thus disposing of that age-old and vexatious problem of the infinite. Bell's erudition here is faulty in that he is unaware of the prior non-Aristotelian rebellion in Hegel's dialectic (whose "verbalizing," incidentally, helped not a little to produce the "doings" of the Marxian "operational" state of Soviet Russia).

Bell's book is stimulating, iconoclastic and devastating. Gilded with literary iridescence and sparkling with original insights, the book takes the new logic seriously and nonchalantly transcends any inherent contradictions. For example, Bell at times speaks of "common sense" in eulogistic terms, only to show, e.g., that Einstein was led to the special

theory of relativity by violating the "common sense" idea that two events can happen in *different* places at the *same* time. However, it may be that mental atavists are unable to follow Bell at all points because they can not translate the "consistency" of the law of excluded middle into the multiple-valued logics of the non-Aristotelian avatars! But whatever one may think of the logic of Bell's intellectual fugue in A flat ($\bar{A}$ logic), it must be admitted that the literary-histrionic qualities are beyond cavil.

And now a word of warning to the author. He has put himself on the spot, so let him guard carefully his protoplasmic integrity! Bell hoists many heads upon the petard of ridicule. He derides philosophers because, in their treatises, they don't use their terms (e.g., "consciousness") in the same sense, but does he suppose that they are such fool-osophers as to overlook the recurring reference, e.g., to "sanity" (pages 53 and 210), with no pretense at a definition? Perhaps the requirement that words be defined and retain constant meanings in their respective universes of discourse is an application of one of the three Aristotelian laws (the "law of identity"), and therefore one of the "laws" we can do without. Indeed, Bell *does* appear to approve Korzybski's attack upon the law of identity (p. 239). Will Bell or Korzybski therefore please tell us what name they propose as a substitute for the principle of the constancy of meaning of symbols? Or do they hold that in the space-time manifold of relativity no organism can ever occupy the same ("identical") space-time on two successive point-instants, and therefore can never use the "same" word (symbol) twice in an "identical" sense? I am really serious about this request, since this is the main stumbling block the reviewer encounters in accepting a non-Aristotelian logic (as pointed out in a paper on "Non-Aristotelian Logics" in the *Monist* for January 1935).

In evaluating Bell's book as a serious contribution to a science of rigid, straight thinking at least two questions arise. The most fundamental question has to do with the *possibility* of a non-Aristotelian logic. Professor C. I. Lewis (referred to in the book) claims that such a logic exists (cf. *Symbolic Logic*, by C. I. Lewis and C. H. Langford), whereas Professors M. R. Cohen and E. Nagel, in their text, *An Introduction to Logic and Scientific Method*, state: "We do not believe that there is any non-Aristotelian logic in the sense in which there is a non-Euclidian geometry. . . ." The reviewer formerly held this same view in his *Humanistic Logic*, but it now seems to me that the view expressed by Henry Bradford Smith in his review of the Cohen-Nagel text is correct

(cf. THIS JOURNAL, October, 1934). On that point the non-Aristotelian doctrine is established.

The other question has to do with the problem of implication. Bell apparently accepts Wittgenstein's thesis that deductive reasoning consists in the elaboration of strings of tautologies (pages 23, 77, 240). Now it *can* be argued that nothing should occur on the right side of a sign of equality, or implication, which does not appear on the left side (allowing of course for substitutions of equivalent symbols), and if anything *does* come out in the deductions or implications which was not in the original assumptions, the inference is invalid. Nevertheless the fact is that the postulates *are* fertile, they *do* generate theorems, and thought *does* advance. How does this logical novelty emerge? Bell himself confesses (p. 214) that he has the optimistic faith that "only those things have human survival value which enable human beings to think consistently and creatively." But how does *creative* thought get started and how does it justify itself? Émile Meyerson has tried to answer this question in his *Du cheminement de la pensée*, but Bell does not give us a theory of knowledge. Bell does suggest (p. 262) that the new logics generate their own mathematics, which furnish mathematical physics with new "explanations" of the universe, to be tested experimentally. But how explain the actual *success* of the natural sciences in using thought structures, if the postulates are a result solely of "convention" and "convenience?" This problem, like the dispute between the mathematical *intuitionists* (Brouwer) and the *formalists* (Hilbert), is not treated adequately. Presumably, however, one must not ask too much of an author, so let us thank Professor Bell for what he has given us and express the hope that he will someday give us the sequel to this adventurous book.

OLIVER L. REISER.

Philosophy Department,
University of Pittsburgh.